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A STUDY OF THE PROBLEM OF AERIAL NAVIGATION, AS AFFECTED BY RECENT MECHANICAL IMPROVEMENTS.

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From Selected Papers of the Institution of Civil Engineers.

In a few remarks appended by the author of this paper to the discussion on Mr. Thornycroft's communication "On Torpedo Boats and Light Yachts for High Speed Navigation," he ventured to express the view that the remarkable reduction lately effected in the weight of power-producing apparatus, might have an important influence on the solution of the problem of the navigation of the air. He considers it may not be out of place, as a matter of mechanical investigation, that he should offer to the Institution some account of the facts and reasonings on which this view is founded.

The serious discussion of the possibility of commanding locomotion at will through the air is often avoided from the fear of encountering popular ridicule. But the engineer and the student of mechanical science will know that there is nothing unreasonable or inconsistent with mechanical principles in the idea. The problem of producing motion in a given direction through the air is analogous with that of producing motion in a given direction through the water, and is subject to the same general laws. Hence, as the latter problem has been long ago practically solved, one may fairly inquire how far the former one is likely to admit of solution also.

The complete form of the problem of aerial navigation is, of course, that of flying, and the study of the mechanical conditions of that wonderful process is one of the most interesting offered by nature. But as hitherto no approach has been made to any artificial imitation of it, its discussion would be out of place here; and it is proposed to confine attention to a modified form of the problem, in which one of its chief difficulties has been removed. The invention of the balloon, about a century ago, overcame the great obstacle to aerial operations caused by the action of gravity, and so immensely simplified the conditions to be studied, as to bring the problem much more within the reach of practical skill. It is therefore to aerial navigation by means of balloons that this paper applies.

The analogy between motion in water and in air has already been pointed out; and it becomes closer when the aeronautic apparatus has the power of floating. Now it is known by every-day experi-

ence that if, in the case of a boat or steamer, an action can be applied, by a force within the vessel, against the surrounding water, the reaction will propel the floating body in an opposite direction; and similarly if a force carried in a balloon can be made to act against the surrounding air, it is equally certain that a propulsion in the opposite direction will be given to the balloon.

And it follows that if motion can be given through the air, there will also be a *steering* power; for the well-known contrivance of the rudder will be as effective, if properly proportioned, in the rarer as in the denser medium. Hence a balloon thus constituted will be capable of navigating the air in any required direction, or will be (to borrow a very appropriate term from the French) a *dirigible* balloon.

The problem, then, in regard to such a balloon is, to ascertain by what means an action can be caused against the air by some force within the balloon itself; and to investigate the result of this force in effecting the propulsion.

The discussion of this problem now to be offered is of no speculative character, and contemplates no novelty of invention. It will be based entirely on existing facts, and on trials made on a full practical scale, which will furnish the data for reasoning on the future possibilities of aerial navigation. Hence it is proposed (I.) To state what has been done; (II.) To infer from this what may be done; and (III.) To offer some considerations on the subject of a practical character.

I. WHAT HAS BEEN DONE.

It is worthy of record that the analogy between water and air navigation was perceived by a great mind, at the time the balloon was invented. As early as December, 1783, *i.e.*, only six months after Montgolfier's first public experiments, Lavoisier, the most eminent chemist and physicist of the day, gave before the French Academy an admirable *resume* of the conditions which should be fulfilled in aerostatic machines, and which are as perfectly applicable now as they were then. In studying the subject he saw clearly that, by reaction against the air, an independent motion might be given to the balloon, and might be made use of to modify the direction impressed upon it by the wind, or in other words to render it dirigible. Accordingly, the last of his conditions ran thus:

"Finally, by employing the force of men, it appears certain that it will be possible to cause the direction of the balloon to vary from the direction of the wind, under an angle of several degrees."

Lavoisier's idea was discussed by the Montgolfiers, who proposed to adapt oars to their balloons; and other early aeronauts from time to time made experiments in the same direction; but none of these efforts were successful. Hence the great expectations which had been raised as to the new power of locomotion gradually dwindled away, and an opinion set in that aerial navigation by balloons was, in the nature of things impossible. This view prevails widely at the present day, and it is not unusual to see the most preposterous and unmechanical notions gravely put forward in support of it. But the explanation of the failure of the early attempts is obvious enough; it lies simply in the difficulty of finding any adequate means of applying the power. Oars were unsuitable with total immersion, and no mechanical ingenuity could imitate the beautiful action of a fish's fin, or a bird's wing. To make the balloon a manageable locomotive agent required a degree of advancement in mechanical practice which has only been attained in very recent times.

It was not till half a century after the invention of balloons that the introduction of the screw propeller removed the first difficulty, by providing an efficient apparatus for acting against the air. This apparatus was at once of the simplest character, suitable for total immersion, easily worked, and capable of applying, in the most effectual way, almost any amount of power that could be desired. After its introduction the practicability of aerial navigation could be no longer doubtful.

The first person who made a serious attempt to utilize the screw for balloons was a young French engineer whose name has since become famous in the engineering world on other grounds, M. Henri Giffard, the inventor of the "Injector," one of the most elegant contrivances ever introduced into engineering. It was about 1850 that M. Giffard turned his at-

tention to the matter, but he found there was much to be done before the experiment could be carried out with any chance of success. In the first place he saw that the ordinary form of the balloon, namely globular, was very unsuitable when lateral motion through the air had to be effected; the well known analogy of vessels for water navigation demanding that the shape should be elongated, diminishing at the bow and stern. To complete the analogy, it was also necessary that this elongated vessel should have a keel and a rudder. As a power to work this screw, he took the bold step of using a steam engine, adopting, however, ample precautions against fire, among which was the ingenious expedient of turning the funnel downwards, and producing the draft by a steam blast, as in the railway locomotive.

His balloon was 12 meters diameter and 44 meters long. The car was suspended by a net in the usual way, and there was a large triangular sail attached to the stern, serving as keel and rudder combined. The steam engine was 3 HP., and worked a two-bladed screw 3.4 meters diameter, which could be given one hundred and ten turns per minute. The general appearance of the balloon will be seen from the accompanying figures.

M. Giffard ascended from Paris on the 24th September, 1852. Having arrived at a convenient height, he started his engine, and the independent motion produced thereby became at once evident by the prompt obedience of the balloon to the action of the rudder. It was "under way," and could be steered like a ship at sea. He found that the screw gave an independent velocity through the air of from 2 to 3 meters a second, or $4\frac{1}{2}$ to $6\frac{3}{4}$ miles an hour.

He intended to continue his experiments, but he found that, in order to get the best results, many improvements were necessary which would take time. His attention was then occupied on other mechanical subjects, but in 1867 and 1868 he had occasion to construct two large captive balloons, in which were perfected some of the improvements he had in contemplation, in particular the impermeability of the envelope, a more mechanical construction of the valves, and a better and cheaper mode of preparing pure hydrogen.

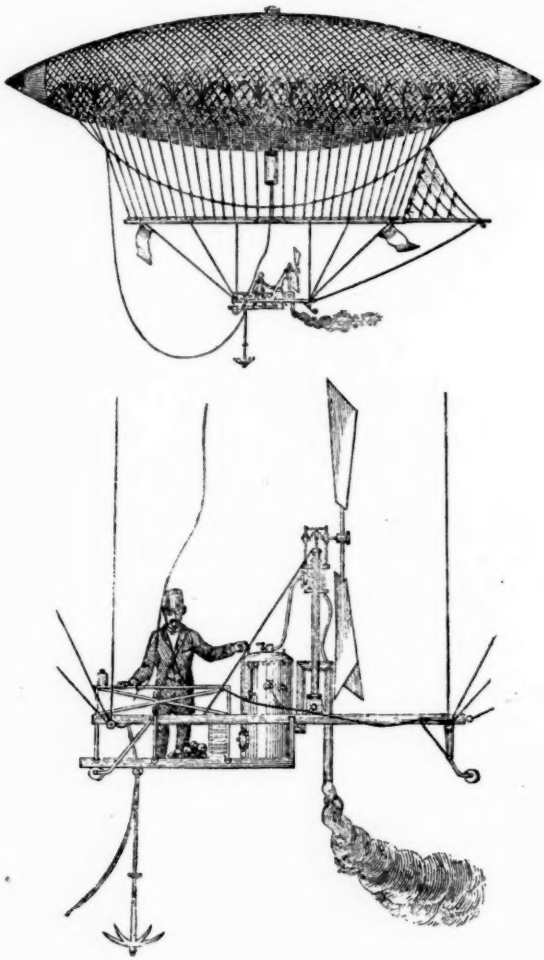
During the siege of Paris in 1870, balloons were used to a large extent, as is matter of history, in order to get despatches out of the city. They were, unfortunately, not available for communication in the other direction; but it occurred to the authorities that if they could be given even a slight independent motion they might be made so, and this led to another experiment under the auspices of M. Dupuy de Lome, the eminent naval architect to the French Government. He constructed a balloon, of an elongated shape, 14.84 meters diameter and 36.12 meters long. The car carried a screw propeller of two sails, 9 meters diameter, intended to be turned by four men, a relay gang being also taken up to relieve them. The experiment was interrupted by the Communist Insurrection, but it was completed afterwards, and the ascent was made on the 2d February, 1872. Careful observations were taken during the voyage, and they established beyond a doubt the efficiency of the propelling apparatus in giving a velocity to the balloon independent of the wind. It was found that when all eight men were working together at the screw, giving it $27\frac{1}{2}$ revolutions per minute, an independent velocity was obtained of 2.82 meters per second, or about 6.3 miles per hour.

As a matter of fact M. de Lome did not accomplish much beyond what M. Giffard had done many years earlier; but his work has a peculiar merit of its own, namely the full and able manner in which, applying to the subject his great knowledge of marine navigation, he has discussed all the elements of the problem. And by the lucid detailed descriptions and explanations he has put on record, both of his calculations and of his experimental results, he has given a firm basis for the extension of the principle to a wider range.

The importance of these two trials, as bearing on the practicability of aerial navigation, cannot be denied; but doubts have been expressed whether the results given can be implicitly accepted. It is said (1) that the determination of the independent speed must be so difficult as to be liable to error; (2) that the results of the two trials, with such different amounts of power, are very discordant, and (3) that had such marvelous accounts been credited at the time they

must have been followed up. In M. Giffard's case, there is, it is true, only the unsupported statement of an engineer of known reputation and great skill; but with regard to M. de Lome's trial, a reference to the "Comptes Rendus" will show abundant evidence of the correctness of his statements. He pre-arranged with great care the modes of observation; he was accompanied and assisted by several other persons, and it is incredible that the full detailed particulars communicated to such a body as the French Academy, by a man of such high position, can have been otherwise than trustworthy. The discrepancy between the two trials will be explained elsewhere; and the apparent neglect of the experiment is easily accounted for by the circumstances of the time, and the want of any sufficient inducement for its renewal. The best answer, however, to

M. H. Giffard's Dirigible Balloon, 1852.



these objections is, that the results are perfectly consistent with mechanical principles, as will now be shown.

II. WHAT MAY BE DONE.

Under this head it is proposed to investigate generally, as a mechanical problem, the capabilities of balloons for aerial navigation.

Assuming that a suitable elongated shape, of circular section, has been determined on, let the maximum diameter be represented by d, and the length by l. Then the contents will be proportional to $d^2 l$ and the *ascending force* of the gas may be expressed by $Ad^2 l$; where A is a coefficient coefficient depending on the shape of the vessel, and on the specific gravity of the gas compared with that of the surrounding air.

The *weight* of the envelope will vary as the maximum diameter multiplied by the length; and for the sake of simplicity, one may, probably without much error, apply the same proportion to the net, the car, and all other parts of the structure generally, including the propeller, apart from its motive power. Therefore, using another coefficient to be obtained from experience, the weight of the structure may be expressed by $B\,d\,l$.

Hence the *available ascending power* $= A\,d^2 l - B\,d\,l$, or $= (Ad - B)d\,l$.

Now this available ascending power has to support the weight of

1. The motor.
2. The necessary stores, such as fuel, water, &c.
3. The cargo.

The proportionate weight to be allotted to each of these respectively will depend on various considerations which it is impossible to reduce to any general rule. For the present purpose attention may be confined to the first item, the motor; and there may be allotted to it a proportion of the whole available weight represented by r; so that the weight of the engine, or whatever the motor may be, will be $= r(Ad - B)d\,l$.

If then S represents the weight of the motor for each (useful) HP., then,

$$\text{Useful HP. of motor carried} = \frac{r}{S}(Ad - B)d\,l \quad . \quad . \quad \text{(I.)}$$

The next question is how the power of the motor is to be expended.

The first element in the calculation is the resistance of the balloon to motion through the air. This is a point of great importance, and it will be necessary to treat it more at length hereafter. For the present, it may be safely assumed, in accordance with the analogy of bodies moving in fluids generally, to vary, for moderate speeds, as the square of the velocity, and it may be represented by Xv^2, where X is a coefficient depending on the dimensions and form of the balloon.

The HP. necesssary to propel the balloon at a given velocity v, will be equal to the resistance multiplied into the velocity, and divided by a certain constant number dependent on the units in which the quantities are taken. Call this H. (For resistance in lbs. and velocities in feet per minute, H = 33,000. For velocities in miles per hour, H=375; in feet per second H=550.)

Hence,

$$\text{HP.} = \frac{Xv^3}{H},$$

which represents the power necessary to propel the balloon through the air.

The next question is as to the *efficiency of the propeller*. This has been often investigated for water navigation. Rankine, in his elaborate article on "Propellers," gives the efficiency of the screw of the "Warrior" = 77½ per cent. Mr. Isherwood makes that of two small boats by Maudslay and Penn = 65½ and 71½ respectively. Mr. Froude reduces it, for high-speed working, to 57½, but this great loss is attributed to causes which would hardly apply to air navigation. M. de Lome estimated the efficiency at 72½ per cent., taking a probable "slip ratio" of 21½ per cent. But as will be hereafter shown, the actual slip in his trial was a little greater, and therefore the efficiency may be put down at 70 per cent., which is fairly borne out by nautical experience. According to this, for every 7 HP. directly expended in propelling the vessel, 10 HP. must be applied to the screw shaft, and the equation becomes—

$$\text{Useful HP. of motor carried} = \frac{10Xv^3}{7H} \quad . \quad . \quad . \quad . \quad \text{(II.)}$$

Equating now (I.) and (II.) and reducing—

$$v^3 = \frac{7rH}{10SX}(Ad - B)dl.$$

If all dimensions are expressed in feet, weights and pressures in lbs., and velocities in feet per second, then H=550, and

$$v^3 = \frac{385r}{SX}(Ad - B)dl. \quad . \quad . \quad . \quad \text{(III.)}$$

An equation which expresses, in compact form, the relations between the chief elements that enter into the problem.

The next step is to obtain the values of the important coefficients A, B, and X.

Ascending power.—Supposing the balloon to be filled with pure hydrogen, the levity of one cubic foot will be = 0.0751 lb. The content of the balloon, according to M. de Lome's proportions, was about 0.434 d^2l cubic feet, so that on this supposition the floating power would be = 0.0327 d^2l. In fact the floating power was = 0.03 d^2l, the difference being no doubt due to the impurity of the gas. The coefficient may therefore be taken at its lower value, *i.e.*,

$$A = 0.03.$$

Weight of the structure.—There is no means of calculating this *a priori*, as it comprehends such a variety of items, dependent entirely on practical considerations. The coefficient must therefore be taken from examples on record. In M. de Lome's balloon the weight was 3885 lbs. = 0.673 dl^2 in M. Giffard's it appears to have been less. The former is the more authoritative, therefore

$$B = 0.673.$$

Resistance of the balloon to motion through the air.—This is the most important element of the whole investigation, and is at the same time the most difficult to determine, from the scarcity of experimental data on a large scale. It is, however, some palliation of the difficulty to know that the resistance of vessels propelled in water is also a quantity liable to much variation and uncertainty, notwithstanding the large amount of experience gained in water navigation. The proper course to adopt here is to apply mechanical analogies as carefully as possible.

The resistance of ships to motion through water may be estimated according to either of the three elements of their dimensions:—(1) The area of immersed midship section; or (2) the skin friction; or (3) the cubic displacement. It will be advisable to apply each of these to the case of the balloon, and see how they correspond.

(1) *By the midship area.* This plan was adopted by M. de Lome, and the following is a *resume* of the way he treated it. He proposed in the first instance to get a velocity of 8 kilometers (4.97 miles) per hour. He took the resistance to a plane surface passing perpendicularly through the air at this speed at 0.665 kilogramme per square meter. But, as is well known, this is reduced in a very large proportion by the pointed form. The elaborate modern investigations of Mr. Froude have shown that, theoretically, the head resistance may be almost annihilated if the most suitable form is adopted; and M. de Lome gives, as a matter of practical experience, the fact of a reduction, in well-formed steamers, to an amount varying between one-fortieth and one-eightieth of the resistance due to the midship section. For his aerial structure, however, he was content to allow a double proportional resistance, taking the coefficient for the balloon at one-twentieth. For the car, accessories, and suspending apparatus he took a coefficient of one-half. This brought out the resistance as follows:—

	Square meters.	Kil.
Balloon.....	$154 \times 0.665 \times \frac{1}{20}$	=5.12
Car, &c.....	$14 \times 0.665 \times \frac{1}{2}$	=4.68
	Total resistance	=9.80

= 21.6 lbs.

This would be the quantity Xv^2 for a velocity of 7.3 feet per second, and a midship diameter of 48.67 feet. From which it follows that the resistance, estimated according to this method,

$$= 0.000171\ d^2v^2.$$

The calculation may be checked in another way. According to the data of wind pressures usually adopted by English engineers, namely, those given by Smeaton to the Royal Society, in his paper on Windmills, the pressure on each

square foot of flat surface $= \frac{1}{437} v^2$, where v is in feet per second.

The area of the midship section will be $= \frac{\pi}{4} d^2$; and that of the car, &c., may be taken at one-eleventh of this. Hence, allowing the same reductions for the form as M. De Lome did, the total resistance—

$$= \left(\frac{\pi}{80} d^2 + \frac{\pi}{88} d^2\right) \times \frac{v^2}{437}$$

$$= 0.000172\ d^2 v^2,$$

agreeing almost identically with M. de Lome's estimation.

(2) *By the skin friction.*—This is a mode which has been sanctioned by recent scientific investigations. Professor Rankine has stated that if W = wetted surface of a ship in square feet, the resistance in lbs. may be taken as $= \frac{C W (\text{speed in knots})^2}{100}$ where C is a constant something greater than unity, whose exact value depends on the lines of the vessel. For the "Warrior," 9,000 tons, he found it = 1.275; for the "Fairy," 168 tons = 1.124. Taking the higher value and putting v = the speed in feet per second, the resistance will be

$$= \frac{W v^2}{224}.$$

Now if air be substituted for sea water the resistance will be diminished in the ratio of the densities, *i.e.*, 793 to 1; and further, the surface of the balloon exposed to the friction of the surrounding fluid may be taken as proportionate to $d\,l$; in M. de Lome's structure it was about $= 2.3\,d\,l$. Hence on this mode of estimation, the resistance for the balloon, taken on the same coefficient as the "Warrior," will be

$$= \frac{2.3\,d\,l\,v^2}{224 \times 793}$$

$$= 0.00001295\,d\,l\,v^2.$$

Adopting then M. de Lome's allowance for the balloon, of double the proportional resistance for a good ship, and adding, as he also does, 88 per cent. for the car, &c., the resistance comes out according to this mode of estimation

$$= 0.0000477\,d\,l\,v^2.$$

(3) *By the displacement.*—This mode combines both the former elements of midship section and skin surface. If D = displacement of a vessel in tons, and v her speed in knots per hour, then the rule given is

$$\text{Resistance in lbs.} = C \times v^2 D^{\frac{2}{3}}$$

where C is a coefficient varying from 0.8 to 1.5, according to the form and condition of the ship. Taking C = 1 for a moderately good example, and changing D to cubic feet, and v to feet per second, the resistance is

$$= \frac{v^2 D^{\frac{2}{3}}}{30.5}.$$

The displacement of the balloon has been given already as $= 0.434\,d^2\,l$, and proportioning for the densities of air and sea water, the resistance becomes

$$= 0.0000238\,d^{\frac{4}{3}}\,l^{\frac{2}{3}}.$$

Increasing as before, and adding for the car, &c., it is

$$= 0.0000886\,(d^2\,l)^{\frac{2}{3}}\,v^2.$$

These three values of the resistance may be compared in the case of any balloon where the proportion of length to diameter is given. In M. de Lome's balloon, for example, $l = 2.43\,d$. Substituting this and reducing, the resistance becomes, when estimated

By midship section	$= 0.000172\,d^2\,v^2$;
"skin friction	$= 0.00011\text{€}\,d^2\,v^2$;
"cubic displacement	$= 0.000160\,d^2\,v^2$.

The estimation by skin friction is the smallest, for the obvious reason that in this structure the proportion of length to transverse dimensions is so much less than is usual in ships. The general comparison, however, shows that the estimate by midship area adopted by M. de Lome, is fairly corroborated by other methods quite independent, and it may therefore be safely taken as representing the resistance.

It is now possible to apply the formulæ to M. de Lome's case, and see how the results correspond with those of experiment. The values of S and r must, however, be first obtained from his data.

The motive power he used was eight men, and he states that, when they were all working together, they produced eight-tenths of a horse power. The men weighed 1325 lbs., which gives—

$$S=1656.$$

And as his total available ascending power was 2,046 kilograms=4515 lbs., the proportion r allotted to his motor was $\frac{1}{4}\frac{3}{5}\frac{2}{1}\frac{5}{6}=0.3$ nearly.

Returning now to equation III., and making $l=2.43\ d$, and $X=0.000172\ d^2$, it becomes—

$$v^3=440{,}000\frac{r}{S}(Ad-B).$$

Wherefore, inserting the values of A, B, r, and S, previously given, the velocity comes out=9.2 feet per second, or

=6.25 miles an hour,

which is almost identical with the speed actually obtained on the trial.

This agreement of the calculated and the observed velocities shows, in the first place, that the result obtained by M. de Lome is in perfect accordance with what might be expected according to ordinary mechanical laws; and secondly, it gives a practical warrant for the more extended application of the reasoning. It is clear that since the power exerted is known, the estimate made of the resistance must hold good, at any rate for moderate velocities; and although there are no experimental data for higher speeds and greater power, yet the analogy of experience in marine engineering will justify the wider application of the rules, if the principles on which they are constructed are sound.

It is therefore proposed to examine what might be expected to be the performances of dirigible balloons, if, in the provision of their power, due advantage were taken of the most recent improvements in mechanical engineering.

It will be evident that the kind of power used by M. de Lome was exceedingly disadvantageous, by reason of its great weight. He fully admitted this, but his object was a limited one, and, under the circumstances, he took, no doubt, the wisest mode of attaining it; for an independent velocity of a few miles an hour would, by taking proper advantage of the wind, certainly have sufficed to enable balloons to enter the city. For more extended applications, however, human power is out of the question, and it is necessary to go back to M. Giffard's plan of using steam, with which, for this purpose, no other kind of motor at present in use could compete.

But although steam power is lighter than that of men, still down to a late period it has been too heavy to be of any real utility in a case of this kind, where the carrying capability is so limited. According to the usual practice with engines used for steam navigation, it may be reckoned that the motor employed has weighed 4 to 5 cwt. per HP., which is also about the weight of small fixed engines in the ordinary market at the present day. At this rate the amount of power which could be carried in a balloon would be so small as not to do much towards the successful solution of the problem of aerial navigation.

But recent improvements have much changed matters in this respect; for in cases where economy of weight has been desirable, the skill of engineers has succeeded in effecting it to a very remarkble extent. In the modern locomotive, for example, much has been done to increase the power that can be developed by an engine of a given weight, and if those parts are excluded which properly belong to the vehicle, and not to the engine, the weight would probably come out not more than about 1 cwt. per HP.

But even this has been much improved upon within the last few years, as will be seen by the paper by Mr. Thornycroft, already referred to. It shows that in the arrangements of power for the light boats there described, the author has succeeded in bringing the weight of the whole propelling machinery down to 43.5 lbs. per indicated HP.; which, omitting the screw and its long shafting and bearings, would probably give not much more than 40 lbs. for the motor alone. In the discussion which followed the reading of the paper, opinions of high authority were expressed that further reductions were possible, particularly in regard to the boiler; but the figure already obtained will suffice for the present object.

It is, however, necessary, in order to make this correspond with the terms of the forgoing formulæ, to transform it into the weight per *useful* HP. The loss between the power indicated in the cylinders and that available at the end of the crank-shaft varies, of course, in different engines, but it is usually reckoned from 15 to 25 per cent. Professor Rankine estimated the loss on the en-

gines of the "Warrior" at 22½ per cent.; Mr. Isherwood made that of Maudslay's and Penn's engines 13 and 14½ per cent. respectively. Mr. Froude estimated it higher, namely, 33.3 per cent.; of which 7.1 per cent. was due to the several pumps. In engines of the light and simple character of those here contemplated, without any air, bilge, or condensation pumps, probably 20 per cent. allowance would be ample; *i. e.* for every 4 HP. applied to the screw shaft, 5 HP. must be indicated in the cylinders. This brings the weight to something over 50 lbs. per useful HP.

But there is another point to consider. If steam power is used, the weight of a store of fuel and water must be also taken into account in the burden to be carried. The consumption of fuel for the lightest engines is given by Mr. Thornycroft at a little under 4 lbs. per indicated HP. per hour; probably some kind of liquid hydro-carbon might be most advantageous for this purpose, and might also lead to a reduction in the weight to be carried.

The water, however, is at first sight a more formidable consideration, the quantity necessary being from 25 to 28 lbs. per HP. per hour. Such a large addition would, a few years ago, have rendered steam ballooning almost impracticable; but fortunately here again recent improvements have come in aid. The water used in steam engines is not like the fuel, decomposed and dissipated; it is only changed in form, and can be reproduced by cooling. M. Giffard saw this, and with the skill of an accomplished practical engineer he proposed to introduce a system of air condensation. The Abbé Moigno gave, in the "Mondes" of 15 Oct., 1863, an account of various improvements which M. Giffard had then on hand, and the following passage refers to this point:

"The provision of water which it is possible to carry in the air being necessarily very limited, it is desirable to use the same water, by condensing the steam after it has produced its mechanical effect. This new improvement has been carried out as rapidly as the former ones; any of our readers may, whenever they please, see, in the Avenue de Suffren, No. 40, suspended to the ceiling of the workshop, a series of flat tubes offering a large surface, which condense the steam of a 10-horse engine."

The air condenser has been used in this country by Mr. Perkins and Mr. Cradock, and it has within the last year or two been successfully applied by Messrs. Kitson & Co. to tram cars running in the streets of Leeds. It is therefore no longer a mere theoretical possibility, but an accomplished fact in steam engineering. From data the author has obtained it appears that with a moderate surface about three-fourths of the water may be recovered, and that a condenser adapted to this purpose may be estimated to weigh about 20 lbs. for each useful HP. of the engine.

From these data the weight may now be made up more accurately. The weight of the engine, with the condenser, may be taken at 75 lbs. per useful HP., *i. e.*

$$S=75,$$

instead of 1656, as in M. de Lome's balloon.

The store of fuel and water necessary to be carried may be estimated, according to present data, at from 10 to 12 lbs. per HP. per hour; but there is little doubt that this quantity, as well as the weight of the engine, could be reduced if the necessity for doing so should arise.

In proceeding now to apply the formulæ to new cases, it is necessary to determine a proportion of length to diameter. This in M. de Lome's case was made 2.43: in M. Giffard's balloon, it was 3.66. There can be no doubt of the advantage of length in diminishing the proportion of resistance to capacity, and in giving better steering properties; and even M. Giffard's proportion (which he found answer perfectly well) is very small when compared with those common in water navigation. In the following calculations, therefore, the proportion $\frac{l}{d}=3\frac{2}{3}$ will be adopted.

This will lead to a new comparison of the estimated resistance, as determined by different methods. By substituting the value of l in terms of d in the various resistance equations, it will be found that the following values appear—

By midship section $=0.000172d^2v^2$;
By skin friction $=0.000175d^2v^2$;
By cubic displacement $=0.000211d^2v^2$.

Here, it will be observed, the effect of the increased length is to bring out higher values of the resistance according to the two latter modes of estimation. On this ground it will be safer to adopt them in preference to the former; and in the absence of any special experience as to which of the two is the more applicable, the mean may be taken, *i. e.*

$$X = 0.000193 d^2.$$

It is further necessary to determine r, the proportion of ascending power to be devoted to the motor, and this may be conveniently made one-third. A sixth may then be added for a store of fuel and water, which would suffice to keep up the maximum power for three or four hours, but would last much longer under ordinary working, when advantage would be taken, to the utmost extent possible, of the direction of the wind. (This store of consumable material might take the place of the ballast used in ordinary aerostation.) The remainder of the net ascending power, one-half, would be available for cargo.

It may be advisable to add to the constant B, to allow for some increased weight that may probably be necessary in the propeller, to meet an increase of power and speed. Instead of 0.673, let

$$B = 0.72,$$

an increase of 7 per cent. on the whole weight of the structure.

Substituting the above values in equation III., it becomes, in round numbers, for the maximum possible speed through the air—

$$\left.\begin{array}{l} v' \text{ in feet per second} = 975(d-24) \\ v' \text{ in miles per hour} = 313(d-24) \end{array}\right\} \text{(IV.)}$$

It remains to say something of the necessary size and velocity of the screw propeller. This instrument must, no doubt, be large, owing to the comparative rarity of the medium against which it is to act; but an idea may be formed of its proportions according to the analogy of water navigation.

In regard to the diameter, the usual rule is to make the area of the screw circle proportional to that of the immersed midship section. M. de Lome states that the most favorable proportion, for good ships, is $\frac{1}{4}$; but considering the increased coefficient of resistance which he had allowed for his vessel, he fixed the diameter of his screw at 9 meters, which gave a proportion to the area of $\frac{63\frac{1}{2}}{168}$ or $\frac{1}{2.65}$. In English steamers, the proportion varies a great deal, but it may generally be taken as from $\frac{1}{2.5}$ to $\frac{1}{3.5}$. M. de Lome's screw was very nearly three-fifths the maximum diameter of the balloon, and, in default of any experience to the contrary, this proportion may be retained.

In order to calculate the velocity of rotation, it is necessary to estimate the amount of slip. In M. de Lome's trial, the pitch of the screw was 8 meters, the number of revolutions $27\frac{1}{2}$ per minute, and the speed of the balloon 169.2 meters per minute. Hence the advance of the vessel for each revolution was 6.15 meters, giving a "slip ratio" of $\frac{1.85}{8}$, or about 23 per cent.

M. de Lome's pitch was eight-ninths the diameter, but this is unusually fine, the general ratio varying from 1 to 1.5. With steam power, no doubt the pitch might be advantageously increased, but in the absence of experience it may not be advisable to depart too widely from what has been done, and the ratio may be put = 1. M. de Lome originally proposed this pitch, and why it was reduced he does not explain.

Calculating on the above slip and pitch, if n = revolutions per minute—

$$n = \frac{78v}{\text{diameter of screw}},$$

or, reverting to equation IV.—

$$n = 1{,}160\frac{(d-24)^{\frac{1}{3}}}{d},$$

which will give the number of revolutions for the maximum speed of any diameter of balloon on the data before named.

Returning to equation IV., the expression shows that a certain magnitude of balloon is necessary to obtain any power of navigation, and that the capability will increase with the diameter. Some different sizes may be calculated in order to illustrate the application of the formulæ, and the results are shown in the following Table.

Dirigible Balloons.

As calculated from data in accordance with the actual trials of Messrs. Giffard and Dupuy de Lome, combined with the results of the most recent improvements in steam motors.

Maximum diameter	Feet. 30	Feet. 40	Feet. 50	Feet. 75	Feet. 100
Length	110	147	183	275	367
Total ascending force	lbs. 2,970	lbs. 7,040	lbs. 13,750	lbs. 46,400	lbs. 110,000
Weight of structure	2,370	4,220	6,600	14,850	26,400
Available ascending force	600	2,820	7,150	31,550	83,600
HP. of motor	3	12	32	140	370
Weight disposable for cargo, after allowing for fuel and water	Cwt. 2½	Cwt. 12½	Cwt. 32	Tons. 7	Tons. 18½
Maximum speed through the air, miles per hour	12	17	20	25	29
Diameter of screw, in feet	18	24	30	45	60
Revolutions per minute for maximum speed	76	81	77	64	55

The smallest size of balloon that would be of any use would be about 30 feet in diameter. This would carry an engine of about 3 HP., giving a maximum speed of 12 miles an hour. The weight available for cargo would be, however, only about sufficient for one person.

Next take 40 feet diameter, the size of M. Giffard's balloon. This would carry 12 HP., would attain 17 miles an hour, and would carry 12½ cwt. of cargo. M. Giffard's engine was only 3 HP., but his balloon was inflated with common coal gas instead of hydrogen, and was therefore deficient in ascending force. The power he had ought to have produced a speed of 10 miles an hour; the reason his result fell so much short of this was the small size of the screw, which was only about one-fifth the proper area, and was therefore quite unable to utilize beneficially the power employed. It is well known, in water navigation, that the loss by slip increases largely when the screw is unduly reduced in size.

The next example is about the size of M. de Lome's balloon, 50 feet diameter, and the calculation shows what it would have done had he used more favorable proportions, and availed himself of the modern steam power. He could at this rate, have carried an engine of 32 HP., which would have turned his screw three times as fast, and would have given him, with the higher pitch, a speed of 20 miles an hour.

By increasing the diameter to 75 feet, the balloon would have a velocity of 25 miles per hour. Even 100 feet diameter would not be an unreasonable magnitude, and this, keeping the same proportion of power to weight, would give a speed through the air approaching 30 miles an hour, and would have 18½ tons disposable for cargo.

These are no doubt startling results, but they arise legitimately from the data now in existence, and it will be seen that their significance, in giving a new aspect to the problem of aerial navigation, is largely due to the mechanical improvements effected in quite recent times. Before the invention of the screw propeller, there were no feasible means whatever of attacking the problem; and even after Giffard and Dupuy de Lome had shown how the screw might be applied, it was not till within the last year or two that the weight of the motor and its stores had been so reduced as to give any hopeful prospect of useful results. That there is now such a prospect, so far as mechanical reasoning can justify it, hardly admits of a doubt.

PRACTICAL CONSIDERATIONS.

It only now remains to inquire into

some of the more important considerations bearing on the question in a practical point of view. And these divide themselves into two classes:—first, as to the construction of the balloon, and secondly, as to its use.

In regard to the first head, the provision of the gas, and its preservation in an envelope that shall be at once light, impervious, and strong, are conditions of ordinary study for balloons generally. M. Giffard devoted much attention to them, and the large captive balloons he constructed were filled with hydrogen at a very moderate cost, which was retained for a long period with scarcely any loss. M. de Lome also considered his arrangements in this respect satisfactory. All other matters of a strictly aeronautical character, may safely be left to the many eminent experts in the art.

But for this purpose an unusual form of balloon is necessary, and important questions arise as to its stability. M. de Lome, with his great experience in analogous questions in naval architecture, saw the importance of this point, and took great pains to investigate the problem. His reasonings may be found fully detailed in the "Comptes Rendus," and it will suffice here to say that he not only determined the stability theoretically, but found his expectations fully borne out by the result of his trial. M. Giffard before him had had doubts on the subject, but adds that his experiment had fully reassured him, and had shown that the use of an elongated balloon was in all respects the most advantageous possible.

As an instance of the care bestowed by M. de Lome on the mechanical design, one contrivance is worth mention. As a balloon rises or falls, the contained gas expands or contracts in bulk, by reason of the variation in the atmospheric pressure. With the ordinary globular balloon the envelope is only partially filled at starting, and room is left in the lower part for the expansion. But with a navigable balloon it is desirable that the external shape should be maintained smooth and unalterated at all elevations. This M. de Lome accomplished by taking advantage of a suggestion made by General Meusnier at the end of the last century, namely, by putting inside the balloon an air pocket, or reservoir, the expansion or contraction of which would compensate for any difference in the bulk of the gas caused either by variation in height or by loss in escape or leakage. This internal vessel was controllable from the car, and it might be given a more extended application in regulating the vertical movements of the balloon generally. M. de Lome states that the behavior of his balloon, not only as to stability, but as to ease of management, was all that could be desired.

In regard to the propelling apparatus, the design of a suitable steam motor would be only a simple task to mechanical engineers accustomed to work of the kind. The construction of the propeller itself would involve more difficulty, owing to the absence of experience on any large scale of power and speed; for in large balloons it must be of considerable size. M. de Lome made one of 30 feet, which appears to have answered very well for his small speeds; but with the higher velocities the thrust would be, of course, increased. The 30-feet screw, when propelling at 20 miles an hour, would have to convey a thrust of about 360 lbs., and this would require a corresponding increase of strength. For the largest balloon in the table the screw must be 60 feet diameter (about the usual size of a windmill) and it would convey a thrust of about 3,000 lbs. The design and construction of such screws, so as to make them combine the necessary strength with the necessary lightness, would no doubt call for considerable mechanical skill.

There is also another point requiring attention, in regard to the position of the screw. To maintain perfect stability during the propulsion through the air, the propelling force ought to act in a horizontal line with the center of all the resistances, which would be a little below the line of the axis. When it is placed lower, there results a tendency to throw the balloon a little out of level. M. de Lome calculated this, and found the deflection was, in his case, less than a degree, which was inappreciable. At higher speeds it would be increased, and probably, with a 100-feet balloon, propelled at 30 miles an hour, it might amount to several degrees, and its effect would require correction in some way.

An arrangement must also be made to

meet the disturbing effect of the loss of weight by the consumption of fuel and water, without wasting the gas; probably M. de Lome's internal pocket might be made useful for this purpose also.

These are, however, after all, only matters of practical mechanics, and one cannot doubt the ability of engineers of the present age to deal with them satisfactorily if the requirement should arise. On the ground, therefore, of practical construction, there appears no reason to doubt the feasibility of carrying out the principles arrived at by theoretical considerations. It is possible that by practical necessities the estimated weights or resistances might be somewhat increased; but there is considerable margin for this, and it must be borne in mind that all the data have been taken on things as they are. When the whole arrangement came to be carefully studied and tried, it is certain that improvements would take place, and what might be lost in some particulars would probably be recouped in others.

But, assuming that dirigible balloons can be constructed, it is desirable further to inquire what practical considerations might affect their use.

It is hardly necessary to say that the introduction of a locomotive machine which would transport a large number of people through the air, in any direction required, at the rate of 20 or 30 miles an hour, would be a remarkable novelty,. and would offer many advantages. Comparing it with ships and boats, it would be far swifter, much less expensive in first outlay and cost of working, would require no harbors, would produce no sea-sickness, and would escape the greatest dangers inherent in water navigation. As a means of land transport, it would be quicker than common road traveling, and would compare fairly with the ordinary speed on railways, while it would dispense with the costly provisions requisite for both these modes of getting over the ground, and would be free from the multitude of liabilities to accident attending them.

But it may naturally be objected that such a mode of locomotion would have peculiar dangers of its own. No doubt balloons have hitherto been very subject to accidents, and the bare idea of anything going wrong at a height of thousands of feet above the earth is very appalling. But much of this impression will vanish before common-sense reasoning. It must always be borne in mind that for the purpose of locomotion there would be no reason for ascending high into the air; it would only be necessary to keep at a sufficient altitude to clear terrestrial impediments, and this would not only do away with much of the terror of the idea, but would greatly increase the probability of a safe escape from accidents of whatever kind.

It is worth while to consider in what direction danger might, in extreme cases, lie. The loss of gas, by rupture of the envelope or otherwise, is a remote possibility; but the experience of many actual cases has proved that the resistance of the air to the large surface exposed has sufficed to prevent any rapid fall. Special measures might be easily provided, and at low elevations over land no serious catastrophe need be feared on this ground. In crossing over water precautions would still be possible, and the case would not be so hopeless as in many marine casualties. The danger of fire, if properly guarded against, need not be greater than in a ship at sea. Indeed, M. Giffard, who has tried the experiment, expressly states that the idea of such danger is quite an illusion.

The accidents that arise to ordinary balloons almost always occur in the descent, which, if the wind is high, requires great care and skillful management. In this case the propelling power would be most especially useful; the aeronaut could choose his place of landing with precision, and by turning his head to the wind he could avoid the dragging which is so dangerous, and which has so often brought a fatal termination to balloon voyages.

On the whole there can be no good reason to believe that the danger would be more formidable with this than with other kinds of locomotion. One cannot ignore the frightful casualties that so frequently now occur in land, river, and sea traffic; and when it is considered how many of their causes would be absent in the free paths of the air, one may even venture to assert that balloons would be the safest, as well as the pleasantest, mode of traveling.

As a set-off against this, however, there is one great disadvantage attending aerial locomotion, namely the *uncertainty* it must always be liable to, in consequence of the effect of the wind. The course of any floating vessel is naturally affected by the general motion of the medium in which she floats. With water the currents may amount to a few miles an hour; with air they will be much more, so much as seriously to interfere with the locomotive capabilities of the balloon.

According to data gathered from the meteorological reports of Greenwich Observatory for the year 1877, it appears that—

			Miles per hour.		
During	17	days in the year the mean velocity of the wind was between....	0	and	5
"	103	days in the year the mean velocity of the wind was between....	5	"	10
"	127	days in the year the mean velocity of the wind was between....	10	"	15
"	75	days in the year the mean velocity of the wind was between....	15	"	20
"	29	days in the year the mean velocity of the wind was between....	20	"	25
"	10	days in the year the mean velocity of the wind was between....	25	"	30
	361				
"	3	days in the year the mean velocity of the wind was between....	30	"	35
"	1	day in the year the mean velocity of the wind was between....	35	"	40
	365				

The mean over the whole year was 13 miles an hour. At higher levels these velocities are exceeded; but, as has been before stated, if balloons were used for the purposes of locomotion, there would be no necessity for them to travel at any great altitude.

Now the course of a navigable balloon will be, like that of a steamer in a tideway, a compound of its own independent velocity with that of the general motion of the surrounding medium. This can easily be calculated by the ordinary rules of navigation, and the following table shows the manner in which the composition of the two motions will influence the locomotive capability of the moving body. It is formed on the assumption that an independent speed of 30 miles an hour might be given to the balloon, and that the wind blows with velocities varying from 0 to 50 miles an hour. The wind is assumed due north, but the relations will be the same for any other direction.

AERIAL NAVIGATION.

Table showing the speed, in miles per hour, that could be commanded on any proposed course, by a dirigible balloon having an independent motion through the air of 30 miles per hour. Wind supposed due north, blowing with velocities varying from 0 to 50 miles per hour.

PROPOSED COURSE.

Velocity of wind.	N	N.N.E. or N.N.W.	N.E. or N.W.	E.N.E. or W.N.W.	E. or W.	E.S.E. or W.S.W.	S.E. or S.W.	S.S.E. or S.S.W.	S.
Calm	30	30	30	30	30	30	30	30	30
5	25	25	26	27	29	31	34	35	35
10	20	20	22	25	28	33	37	39	40
15	15	15	17	20	25	32	39	44	45
20	10	10	13	16	22	31	41	48	50
25	5	5	7	9	17	29	43	51	55
30	..	..	..	..	..	22	43	56	60
35	..	..	..	..	..	..	42	59	65
40	..	..	..	..	..	..	38	63	70
45	..	..	..	..	..	..	..	67	75
50	..	..	..	..	..	..	..	70	80

The practical result of this would be as follows:

(1.) In storms and gales, say exceeding 40 miles an hour, it would not be prudent for the balloon to travel at all. Ships only sail "wind and weather permitting," and balloons must submit to the same restriction.

(2.) In high winds, say from 30 to 40 miles an hour, it could only go in a course generally corresponding with that of the wind; but it would still have a considerable range of direction and a high velocity, and, what is of the greatest importance, it would have the power of steering, and would be able to command its descent at any time, and in any place, without danger.

(3.) In light and moderate winds, under 30 miles an hour, which the Greenwich observations record to prevail all the year with the exception of a few days, it could travel in any direction,

the speed varying from 5 to nearly 60 miles an hour.

It must also be added that with contrary winds the voyages must be necessarily short distances at a time, from the impossibility of carrying large stores of fuel and water to keep up the full power for any long period. But with favorable winds, such as the trades, almost any distance might be run, as the use of the engine would be limited to what was necessary for steering purposes.

These conditions would no doubt render aerial navigation unsuitable for traffic that requires regular and punctual transit, and would, therefore, much limit its commercial value. It could never, for such purposes, compete with railways, or lines of river or sea navigation. But still a great variety of cases exist where its peculiar advantages would tell in practical use; and probably, if such a means of locomotion were once introduced, increased employment for it would soon arise.

SUMMARY.

The foregoing investigation appears to warrant the following conclusions.

1. The problem of aerial navigation by balloons is one as perfectly amenable to mechanical investigation as that of aquatic navigation by floating vessels; and its successful solution involves nothing unreasonable, or inconsistent with the teachings of mechanical science.

2. It has been fully established by experiment that it is possible to design and construct a balloon which shall possess the conditions necessary for aerial navigation, *i. e.*, which shall have a form of small resistance, shall be stable and easy to manage, and, if driven through the air, shall be capable of steering by a proper obedience to the rudder.

3. If, by a power carried with the balloon, surfaces of sufficient area can be made to act against the surrounding air, the reaction will propel the balloon through the air in an opposite direction.

4. The modern invention of the screw propeller furnishes a means of applying power, in this way, to effect the propulsion; and the suitability and efficacy of such means have been shown by actual trial.

5. Sufficient data exist to enable an approximate estimate to be made of the power necessary to propel such a balloon with any given velocity through the air.

6. The recent great reduction in the weight of steam motors has rendered it possible to carry with the balloon an amount of power sufficient to produce moderately high speeds, say 20 or 30 miles an hour through the air; and by taking advantage of other recent improvements it would also be possible to carry a moderate supply of fuel and water for the working.

7. The practical difficulties in the way are only such as naturally arise in the extension of former successful trials; and such as may reasonably be expected to give way before skill and experience.

8. The practical utility of aerial locomotion must always be considerably restricted by the effect of the wind, which it is impossible for any flying body to evade. But still, such a system would have peculiar advantages of its own; and on the whole, dirigible balloons may form a feasible and useful addition to the present means of transport, and are, therefore, worthy the attention of the engineer.

AS TO THE FUTURE OF ELECTRIC RAILWAYS.

From "The Builder."

THE application of electricity to locomotion is a subject on the exhaustive knowledge of which so much of the future welfare of the human race depends, that it is desirable to refer to those statements by Professor Ayrton on the subject, some of which are to be found in our columns (*ante*, p. 384). Nor is our object in thus doing so much either to support or combat the opinions of the lecturer, as to bring forward some of those considerations which the practical knowledge of our railway system from its very cradle have rendered more familiar to the engineer than to the electrician.

Professor Ayrton has not omitted to point out that the work done in the moving of the locomotive engines forms a

very serious part of the whole work done by our railways. This, no doubt, is so; and that it is so to a greater extent than has been as yet estimated will be seen by what we have to remark.

That the engines on the railways of the United Kingdom travel a much longer distance than the 222 millions of train miles of which the Board of Trade returns yield us the sum, there is, of course, no doubt. In some of the accounts of the companies, the mileage of engines is, or rather was, returned as a separate item from the train mileage; but we find no information on this score in the "Railway Returns" or in the "Index to our Railway System" at present. We are, however, in possession of two sources of information on this subject, to which it may be of service now to direct attention. One of these is the Report on the Railways of New South Wales, which, as published at Sidney, is not by any means so well known in this country as ought to be the case. The other is a series of elaborate tables of the working elements of the Richmond and Danville Railroad Company, which we owe to the courtesy of the general superintendent of that line.

On the New South Wales Railways in 1876 (the latest year for which we have a report at hand), the total number of engines and tenders was 101,—51 being for the passenger, and 50 for the goods traffic. The passenger engines weighed a little over 38 tons, and the goods engines a little over 49 tons each, the weight of the tender being included. The carriages forming the passenger stock weighed a little over 6 tons 1 cwt, on the average, and were 344 in number. The goods vehicles were 3,198, and weighed, on an average, 4 tons 16 cwt. The gross mileage of the engines in the year was 2,160,242 miles, of which 993,522 were run by the passenger engines.

The Government Commissioner for Railways in New South Wales in that year, Mr. John Rae, to whose conscientious appreciation of the duties of his position we owe the above data, has gone a step further in his tables, and has given us not only the materials for calculation, but the outcome of very minute computations. It is not necessary to add very much labor to the published tables to come to the following results:

For the passenger traffic on all the New South Wales lines, in the year 1876, the proportionate weights of engines, vehicles and loads were:—

Engines	51.3
Vehicles	45.3
Loads	3.4
	100

For the merchandise traffic, the corresponding proportions were:

Engines	34.8
Vehicles	42.4
Loads	22.8
	100

The value of statistical information of this kind becomes very great when we enter into such questions as that of the economy possible to be effected by electric power. From 35 to 51 per cent. of the gross work done on these railways consisted in moving the locomotives themselves. But, in addition to this, the disadvantage at which the locomotive works is shown by the difference of the formulæ used to express the resistance to the carriage and to the entire train. For a train consisting of an engine and tender weighing 50 tons, and 100 tons of carriages, the total resistance, at thirty miles an hour on the level, is 3,000 lbs. But the resistance to the carriages alone is only 1,328 lbs. Thus, it is not only in the weight to be moved, but also in the mode of moving the weight, that the loccmotive is so costly, that an economy of 56 per cent. would be secured by dispensing with its use. How much of the proportions of 45 and 42 per cent. of the gross load that is formed by the vehicles is due to the extra strength required for the resistance to locomotive energy is not so obvious.

Turning now to the tables kindly furnished by Mr. T. M. R. Talcott, the general superintendent of the Richmond and Danville Railroad Company, we have somewhat different results, although the difference may probably be accounted for by the lower speed at which the traffic is usually carried on in the United States, as compared to that to which we are accustomed, and by the larger volume of traffic. On the average of the three years, 1875, 1876, and 1877, the proportionate weights were as follow:—

For passenger traffic—

Engines.....	32.80
Vehicles.....	61.53
Loads.....	5.67
	100

For merchandise traffic—

Engines.....	15.85
Vehicles.....	51.98
Loads	32.17
	100

As the New South Wales lines are in an early stage of development, it may be considered that we have here two extreme cases, within the limits of which the proportionate weights will be found to range on different lines. Roughly averaging the above, we find that the weight of the locomotives is about 35 per cent., that of the vehicles 49 per cent., and that of the load 16 per cent. of the total weight moved.

On this view, as far as the mere question of the weight of the locomotive is regarded, it may be doubtful how far the loss of power by electric leakage will serve to counterbalance any economy effected by the abandonment of the engines. But the question of the diminution in the weight of the vehicles has to be borne in mind. As to that, we are not prepared at the present moment to offer a decided opinion. But there can be little doubt that the important item of capital outlay would be enormously reduced, both by the diminution in the strength of the permanent way and of the works of art that would be necessary to carry the traffic, if the heavy engines were abandoned, and in the much greater steepness of the inclines which it would be not only possible, but easy, to work, under those conditions.

We are, further, in possession of information derived from an experience which is now almost forgotten, but which bears very directly on this question. It is now some thirty-six years since Mr. Robert Stephenson designed the mode of working the Blackwall Railway by stationary power. Mechanically regarded, the plan was a success; and a financial result was also admirable. But a practical difficulty arose from the constant twisting and breaking of the rope. And what rendered this so formidable as to lead to the abandonment of the system was the fact, that on the fracture of the rope the whole traffic of the railway, on both lines, was brought to a standstill.

But the most interesting part of this experience is this. The cost per train mile was 1s. 6¾d.; the trains, however, being much lighter than those which on the railways of the United Kingdom now cost an average of 2s. 11d. per mile. Of this cost, however, by far the greater part was incurred in moving the machinery and the rope. Out of 324 indicated horse power, it was found that 251 horse power was thus expended; so that only 63 horse power, or under 20 per cent. of the whole, was employed in the direct traction of the vehicles and load.

The cost, notwithstanding, works out as low as 0.187d. per ton per mile, which we make to be 10 per cent. lower than the average cost of propelling a ton for a mile on the railways of the United Kingdom in 1879. But as the traction of the load and vehicles only absorbed 20 per cent. of this power, we get a cost, for that part of the duty alone, of 0.038d. per ton per mile, or less than one-fifth of the cost of the railway power of today. We do not insist too much on the accuracy of the comparison, because the cost now includes some 30 per cent. in the form of traffic expenses, which were not so heavy on the Blackwall line. Still, on the rough statement that, (1) stationary power is somewhat less costly than locomotive power, even under circumstances unfavorable for the former, and (2) that these circumstances may be so unfavorable as to increase the power required for the traction of load and vehicle alone from 63 to 324 horse power, we think it is tolerably clear that any mode of using stationary power, which can draw a train, saving the weight of the engine, and applying its force in such a manner as not to lose more than 30 or 40 per cent. between the motor and the work, has an immeasurable future before it.

A LARGE Lacustrine canoe has been found at Bex, Switzerland, in a fine state of preservation. Bex is 4000 feet above the sea level, and nearly 3000 feet above the Valley of the Rhone.

THE BASIN AND REGIMEN OF THE MISSISSIPPI RIVER.*

By PROF. C. M. WOODWARD.

THE Upper Mississippi unites with the Missouri River about twenty miles above St. Louis, so that the Mississippi, as it rolls by the city, contains only the waters of those two streams. The basin of the Missouri River includes an area of 518,000 square miles; that of the Upper Mississippi about 169,000 square miles; hence the drainage of 687,000 square miles of the earth's surface forms the river at St. Louis.

The great extent of this joint basin is better appreciated when it is compared with other areas well known. It is eighty-eight times as large as the State of Massachusetts, or equal to the combined areas of England, Scotland, Wales, Ireland, France, Spain, Portugal, Holland, Belgium, Switzerland and Italy. Again, it is equal to the sum of the areas of the basins of the Vistula, Oder, Elbe, Rhine, Seine, Loire, Garonne, Douro, Tagus, Ebro, Guadiana, Rhone, Po and the Danube. It is however probable that the volume of water discharged is not proportionately great.

The basin of the Upper Mississippi is wholly devoid of mountains, though the country is well wooded and abundantly supplied with lakes and streams. The average annual rain fall is 35.2 inches.

The Missouri basin includes the eastern slope of the Rocky Mountains for a length of about 800 miles. From these mountains several large streams issue, and flow for hundreds of miles across the great barren plain with little increase of size. "Comparatively little rain falls upon the mountains and plains, and hence the size of the main river is proportionately small when the drainage area alone is considered."† The average annual rainfall in this basin is 20.9 inches, and that of the two rivers combined is 24.4 inches. The river drainage is less than one-fifth of this average.

The average discharge per second of the Upper Mississippi is given as 105,000 cubic feet, and that of the Missouri as 120,000 cubic feet. Hence the discharge of the river at St. Louis is 225,000 cubic feet per second or 7,080,000,000,000 cubic feet per year. The *maximum* discharge must be at least four times this average.

At the mouth of the Missouri, the Mississippi takes on its peculiar character of a deep and boiling torrent. Its width is increased but not so much as its depth.

The river is subject to great changes both seasonal and irregular. The highest water is during the "June rise" (which may be a month or two early or late), and low water is usually in December. The greatest range ever observed at St. Louis between extreme high and extreme low water is 41.39 feet, the high water being that of 1844 when the water was 7.58 feet above the city directrix. The city directrix is the curbstone at the foot of Market street, and marks the height of the water in 1828; it serves as the datum plane for all the city engineering at St. Louis. The bridge levels are generally referred to the same line. Thirty-four feet below the city directrix is known as "low water."

The velocity of the current where it is greatest, opposite to St. Louis, varies from 4 ft. per second (or $2\frac{3}{4}$ miles per hour) at low water, to $12\frac{1}{2}$ feet per second (or $8\frac{1}{2}$ miles per hour) at extreme high water. The average slope of the water surface is about 6 inches per mile near St. Louis.

At all times the river water is turbid, and when it is allowed to stand a few hours a sediment is deposited; but the amount of matter held in suspension varies greatly. The sediment consists of finely divided vegetable and mineral matter gathered from tributaries through alluvial districts, and from the bed and banks of the stream. In order to appreciate the difficulties to be surmounted in bridging the Mississippi at St. Louis, it is necessary to clearly understand the laws which appear to obtain in the action of the river upon its banks and bed, and so determine its power to transport sedimentary matter.

This "carrying power" has reference

* A History of the St. Louis Bridge, by C. M. Woodward. St. Louis: G. J. Jones & Co.
† Humphrev's and Abbot's Mississippi River.

not only to the amount of sedimentary matter it can hold in suspension but also to the amount of material which under the influence of the impulsive force or momentum, of the water is driven along in a more or less fluid state. The distinction here made is one of degree rather than of kind. Water moving slowly in a smooth, regular channel, can carry very little mineral matter; but, increase its velocity and volume and it will sweep along not only sand and mud, but gravel and large pebbles. When from irregularities in the bed of a stream, the body of the river is full of whirlpools—cross and vertical currents—the action is analagous to that of jets driven by high pressure.

It appears that this transporting power of a river depends upon: (1) The specific gravity of the sediment, (2) the size of the sedimentary particles; (3) the relative or internal velocity of adjacent masses of water; (4) the depth of the stream; (5) the absolute velocity of the stream.

1. Woody fiber and the tissue of vegeetable cells, loam, clay, particles of limestone, sand and gravel form the main burden of the river. The specific gravity varies from 1 to 3.

The specific gravity of the strictly suspended matter is given as 1.9 by Humphreys and Abbot.

2. The size of the particles is very important. The heaviest materials, if in a finely-divided state, may be transported by the running water in rivers. If the particles are supposed to be similar in shape, we easily see that their stability in running water is less as they become smaller. Their weight, and consequently the resistance which they offer to being raised or pushed along by currents, varies as the cube of any one of their dimensions, as, for instance their thickness; while the force to which they are exposed (the pressure or impact of the waters upon their surface) varies only as the square of the thickness. For example take two similar blocks of granite, or two grains of sand, the larger of which is three times as thick as the smaller; the weight and therefore the friction of one is twenty-seven times that of the other; while its surface, and hence the force with which water would press upon or strike it, is only nine times as great. It is evident that the smaller particles might be transported or pushed along, while the larger would stand unmoved. It follows that, for a given current of water, there is a point of fineness for each substance at which the particles become transportable. As a consequence we should expect in a diminishing river current to find the larger and denser particles left behind first, the smaller and lighter next, and so on, the finest and lightest only being deposited where the water is stationary.

3. In a stream full of whirlpools and boils (or vertical currents in opposite directions) the water is intermittently impinging upon the bed and banks. These currents not only prevent the deposit of what would otherwise come to rest on the river bottom, but when not fully loaded with sedimentary material, they seize upon all within their reach and carry it along. So far as velocity in the direction of the axis of the stream is concerned, the greatest "difference of velocity" in adjacent water layers, or masses, is found near the bed and banks of the stream; but where cross and vertical currents exist, the resultant difference in velocity is likely to be greatest, where the onward flow is greatest.

4. The modifying effect of depth on the power to transport solid matter in a sediment-bearing stream is shown in two ways:

In the first place as the depth increases, the internal relative motion of adjacent layers is diminished ("still waters run deep," and conversely); this alone lessens the transporting power. In the second place, the relative motions of a deep stream are powerful, and slowly moving masses of water produce great inequalities of pressure on the materials of the bed. These unequal pressures suffice to keep the loose material on the bottom in constant motion, thus increasing the transportation. A paragraph in Mr. Eads' report of May, 1868, is so pertinent that I quote it here. "I had occasion," he says, "to examine the bottom of the Mississippi, below Cairo, during the flood of 1851, and at sixty-five feet below the surface I found the bed of the river, for at least three feet in depth, a moving mass, and so unstable that, in endeavoring to find a footing on it beneath my bell, my feet penetrated through

it until I could feel, although standing erect, the sand rushing past my hands, driven by a current apparently as rapid as that at the surface. I could discover the sand in motion at least two feet below the surface of the bottom, and moving with a velocity diminishing in proportion to its depth." At Carrollton, gravel, sand and earthy matter were found moving along the bottom at a depth of about 100 feet by Professor Forshey. It is obvious that increase of depth diminishes rather than increases the "suspending" power per unit of volume, though it adds largely to the motive force of the stream.

The absolute velocity of the water is of course a very important matter, both from the momentum with which it strikes all obstacles, and from the fact that increase of absolute velocity always involves increase of relative motion. With a given channel, depth of stream, nature of sediment, there is a maximum load for each velocity, and the load increases as the velocity increases, though the law is not exactly known. The practical limit to the power of water to hold matter heavier than itself in suspension suggests that the solid particles afford each other a sort of protection from the impulsive force of the water, and that the amount of this protection increases as the number of particles in suspension increases, and that at a certain point the protection is so efficient that the water is unable to prevent their fall. This protection is of course mutual among the particles. Thus, if we suppose several grains in contact and in a row, we see that the efficiency of the force is much less than with a single particle, as the surface of action remains the same, while the force to be overcome is increased. As the kinetic energy of the water is proportional to the square of its velocity, it is probable that the law referred to above would prove that the carrying power of a river is, other things being equal, proportional to the square of its velocity.

These main principles, derived partly by theory, and partly by observation, are well confirmed by the behavior of the Mississippi at St. Louis. At "low water" the water is least turbid, the velocity is small, the stream shallow and confined to the main channel. It can carry but little solid matter, and it finds its load in the deposits made during the subsidence of the last flood. This is comparatively heavy material, and settles readily when the water is stationary. When from any cause a rise takes place, the increasing tide seizes upon the lighest and finest materials first, and it is noticed that the suspended matter in samples of water at such times settles slowly and with great difficulty. But the demand of a flood is not easily satisfied. If the water enter the stream comparatively clear (like the Upper Mississippi), it is much undercharged and quickly attacks the old deposits along the river bed, and if the flood is great, it even scours out and carries away sand bars and islands. It is generally true in the Mississippi that changes in level of the surface are accompanied by contrary changes in the bed—*i. e.*, as the surface rises, the bed falls under the erosive action of the flood, and as the surface falls, the bed rises by deposit. The heavier materials are transported with far less than the mean velocity of the stream, and as the flood begins to subside, they are left behind in the form of new bars and alluvial deposits to form new islands.

A flood from the Missouri invariably brings great quantities of matter into the Mississippi; and if at the time the Upper Mississippi is low, the result on the return of the river to its normal flow is a large increase of mud and bars, which under the action of a joint flood, or one from the Mississippi alone, disappears. In this way the bed of the stream is continually changing: but every change is towards the Gulf of Mexico, into which not only the lighter suspended matter finds its way, but ultimately the sand bars as well.

The depth of scour of the river is sometimes very great. An obstacle in mid-channel, like the wreck of a boat, the pier of a bridge, or a thick gorge of ice may serve to give to the current a new direction and increased velocity, forcing it far below the normal bed of the river. In 1854 Mr. James H. Morley, chief engineer of the Iron Mountain Railway, took soundings through the ice across the Mississippi near the site of the present bridge. He found a depth of 78 feet, when the river was only 10 feet above low water. The "line of scour" was thus shown to be at least 68 feet below

low water, instead of 30 feet below, as was assumed by Mr. Boomer's convention of engineers in 1867. Soundings made in 1876 off the east abutment of the bridge where, when the abutment was constructed, the water was not more than 15 or 20 feet deep, showed a depth of nearly 100 feet. The materials of which the bed of the river at St. Louis is composed were seen by borings, and later by the excavation under the bridge piers, to be the heavier debris of river floods. Even the bed rock when laid bare, was smooth and water worn. It is clear that either the mighty river had at one time its normal bed on the rock, or else it has in ages past during its countless floods, again and again scoured down to the rock itself. In the light of these facts, he would be a rash engineer indeed who should place any reliance upon the uncertain footing of the river bottom as a support for the foundations of his bridge.

The river ordinarily freezes over in winter. The ice coating is however generally composed of huge irregular fragments of ice from the North. No sooner does the cold weather set in than the river is full of cakes of ice. Under the influence of intense cold, the cakes freeze together and form large ice fields. These, in some narrow pass or across the head of an island, gorge together, become stationary, and unite into a strong bridge of ice. The surface of the river above is soon crowded full of ice, and the river is closed. During the formation of an ice gorge, large cakes of ice are carried by the current underneath the surface layers to such an extent that the gorge is, at times, a solid mass of 20 feet or more in thickness. The scouring action of the water under such gorges is obvious. Since the erection of the bridge the piers have helped to form an ice gorge above it, leaving the water clear below. This has proved of great value to the navigation of the lower river, and has caused very deep water between and above the piers. Foundations less deep and strong would have been exposed to great danger.

River ice is regarded as very treacherous. Previous to the construction of the bridge, the river would occasionally in mid-winter be closed to boats and teams for days together; sometimes the most daring footman could not cross. At such times when all communication with the East was suspended, when anxious travelers were visible on the other shore, the people of St. Louis earnestly prayed for a bridge which should put them beyond all danger of an "ice blockade." The river has been known to close early in December and remain closed till the latter part of February. After freezing over the water usually rises a few feet, from the action of the ice gorge.

There is something almost sublime in the immense volume and apparently irresistible power of this great river. The ease with which it devours island after island, and forms for itself a new channel; the wild deluge of waters with which, without apparent loss of volume, it covers thousands of miles of fertile fields; and the unequaled strength and depth of the current,—suggest a power so far beyond human control as to seem almost lawless; and yet nothing is more certain than that, in all its moods and phases, it is wholly obedient to nature's laws, and that the engineer who would grapple with the problems involved in the practical management of the Mississippi must study and master those inflexible ordinances.

Said Charles Ellet forty years ago: "The power of this great river does not prohibit any attempt to restrain, to force, or to change its current; on the contrary, it may be almost wholly subject to the control of art. Apparently, it varies its depth, alters its direction, reduces or increases its width, with regard only to its boundless power; but these movements are all made in obedience to certain laws, uniform and universal in their action, to the rule of which it is as completely subject as any other effect in nature to the cause by which it is produced. To govern it the labor of man must be applied with a knowledge of the influences which it recognizes; and that power which renders it apparently so difficult to restrain may then be made the means of its subjection."

While Ellet thus wrote, James B. Eads was studying the habits of the river from the deck of a Mississippi steamboat, or on the bed of the river under a diving-bell. Over thirty years later, after an intimate acquaintance with the river for nearly forty years, Mr. Eads eloquently

gave utterance to the same thought: "My experience of this current has taught me that eternal vigilance is the price of safety, and constant watchfulness is one of the first requisites to insure success, almost as much as knowledge and experience. To the superficial observer, this stream seems to override old established theories, and to set at naught the apparently best devised schemes of science. But yet there moves no grain of sand through its devious channel, in its course to the sea, that is not governed by laws more fixed than any there were known to the code of the Medes and Persians. No giant tree standing on its banks bows its stately head beneath these dark waters, except in obedience to laws which have been created in the goodness and wisdom of our Heavenly Father to govern the conditions of matter at rest and in motion.

"It was necessary for this young engineer* to master these laws before he dare attempt to plant one of these stately piers. Once assured by careful study, patient experiment and close observation that he was applying those laws rightly to accomplish his end, the vagaries of the stream were to him as easily comprehended, and as simple as the ordinary phenomena of every-day life. No halfway knowledge of the laws which control this ceaseless tide, or govern the effects of temperature, and the strength of materials, would suffice to accomplish what he has done—to place these piers in this river, and to spread across its turbulent bosom, like gossamer threads, this beautiful and strong iron structure, over which the commerce of mighty States is henceforth to roll with speed and safety."

* Col. C. Shaler Smith, Engineer of St. Charles Bridge.

PILE FOUNDATIONS AND PILE-DRIVING FORMULÆ.

From a Circular of the Office of Chief of Engineers,

The following correspondence respecting pile foundations and pile-driving formulæ is communicated to the Corps of Engineers.

The Chief of Engineers approves the suggestions contained in Major Weitzel's letter of the 4th of October, and desires that the officers of the Corps will, at their leisure, communicate to this office any views they may have on the subject of this correspondence, which he deems of great practical importance, and also the results of their experiences with pile foundations.

He also desires that whenever an officer of the Corps has occasion to construct a pile foundation, he will cause to be kept an accurate record of the driving of the piles, embracing the kind, and average size and weight of the piles, the weight and fall of the ram, and the penetration at each blow, or at least at each of the last (say five) blows, a copy of which record he will send to this office with a plan of the foundation, on which is marked the estimated weight each pile is to carry, and also a description of the soil.

By command of Brig. Gen. WRIGHT.

GEORGE H. ELLIOTT.
Major of Engineers.

Abstract of a letter from Major G. Weitzel, on the pile and grillage foundation for the Martello tower at Proctorsville, La.:

The foundation was constructed in 1856 and 1857.

The site of the tower at Proctorsville, as determined by actual borings was found to have the following character, viz.: For a depth of nine feet there was mud mixed with sand, then followed a layer of sand about five feet thick, then a layer of sand mixed with clay from four to six feet thick, and then followed fine clay. Sometimes clay was met in small quantities at the depth of six feet, as well as small layers of shells. By draining the site the surface was lowered about six inches.

The foundation piles were driven in a

square of twenty piles on a side, four feet from center to center. Twenty-four were omitted to leave room for fresh water cisterns, and two extra ones were driven to strengthen supposed weak ones. The total number at first driven was therefore 378. The piles were driven to distances varying from 30 to 35 feet below the surface, or from 10 to 15 feet into the clay stratum. The average number of blows to a pile was 55, and mainly hard driving. After all these piles were driven, ten additional ones were driven at different points to strengthen supposed weak points. Each one of them required over 100 blows to drive it.

Before beginning the foundation I drove an experimental pile exactly in the center of the site. It was 30 ft. long, $12\frac{1}{2}'' \times 12''$ at top and $11\frac{1}{2}'' \times 11''$ at butt. It was sharpened to a bottom surface about 4 inches square. Its head was capped with a round iron ring. Its weight was 1,611 pounds and the weight of the hammer was 910 pounds. Its own weight sank it 5′ 4″, and it required 64 blows to drive it 29′ 6″ deeper. The fall of the hammer at the first blow was 6 feet, increasing each successive blow by the amount of penetration, excepting the last ten blows when the fall was regulated to exactly 5 feet at each blow.

The penetrations in inches were as follows:

12—12—16—11½—10½—10½—8—6—6½—6⅝—7¼—7½—7½—6¾—6⅜—6½—6—6—6—6⅛—6¼—6⅜—6—6—6⅜—6⅜—6—5⅜—4⅜—4⅛—3⅛—3—2⅞—2½—2¾—2⅝—2⅜—3⅛—2⅞—2⅞—3—3—2—2⅛—2⅛—2½—2¼—2⅜—2⅜—2½—2⅝—2⅝—2⅝—2¾—3—⅜—⅜—1—¼—½—⅜—¼—⅜—⅜—⅜.

This pile according to Colonel Mason's formula, should have borne 52,556 pounds. I loaded it with 59,618 pounds and it did not settle. I afterwards increased the load to 62,500 pounds, when it settled slowly. The greatest weight to be carried by any one pile was between 30,000 and 35,000 pounds.

The tops of the piles were sawed off on a level, and the whole surface between them covered with a flooring of three-inch planks tightly fitted in, the upper surface of this floor being flush with the tops of the piles. They were then capped in one direction by stringers $18'' \times 18''$ and 85′ long. Each of these stringers was constructed by splicing two shorter ones of equal length by means of the regular scarf joint. These were bound together by $12'' \times 12''$ stringers 85′ long (formed by splicing two shorter ones) running over the line of piles in the perpendicular direction. These $12'' \times 12''$ stringers were let into the $18'' \times 18''$ so that their top surfaces were flush. In the little squares thus formed, and next to the $18'' \times 18''$ timbers, were laid short pieces $12'' \times 12''$ timbers, and the intervals filled in up to the level of the latter with concrete. The whole grillage was then leveled off with short pieces of $6'' \times 12''$ planks. This grillage was, therefore 18 inches thick. Long sheet piling was driven for the scarp of the wet ditch, the upper ends resting on the inside of the stringers on the outer row of piles.

In order to distribute the weight of the tower uniformly over this foundation, strongly reversed groined arches were turned, the space between their backs and the grillage being filled in with solid concrete masonry.

When the brick work of this tower, which was carried up even on all sides, was about half completed and the foundation had on it less than half the load it was designed to carry, the appropriation became exhausted and the work was stopped. This was in the spring of 1858. When I visited the work about six months thereafter I found a marked settlement. The four salients apparently remained intact, but on every side the settlement was about the same, and largest about the middle, so that the courses of brick which were laid perfectly level had the form of a regular curve.

I was serving at that time as assistant to Brevet Major G. T. Beauregard, Captain of Engineers. In addition to his military works, he was in charge of the construction of the new Custom House in New Orleans, La.

In order to ascertain the cause of this settlement he directed some experiments to be made by the architect of that building, Mr. Roy.

I do not remember the details of these experiments. I was on duty at Forts St. Philip and Jackson, and afterwards stationed at West Point while they were made. The civil war also intervened. Subsequently, however, to the latter, I

met Mr. Roy, and he told me briefly that the experiments proved that piles of different cross sections driven in the same Louisiana soil and under exactly the same conditions, do not have a power of resistance proportional to the area of their cross section, and that the capacity of resistance per square inch in cross-section of pile diminishes as the area of this cross-section becomes greater. That is to say, a pile 4″ square in cross section does not have four times the resistance to pressure of one 2″ square. This decrease, he said, became quite marked as the cross section of the piles increased. He believed that the piling for the foundation at Proctorsville was driven so closely that the whole system assumed the character of a single pile about 81 feet square in cross section, and that therefore its capacity of resistance per square foot was very much reduced as compared with the capacity of resistance per square foot of my experimental pile.

I have never since had an opportunity to test the accuracy of this conclusion, but I believe that some of the officers of our corps are so situated that they can do it, hence this communication.

From a second letter from Major Weitzel to Brigadier-General Wright:

The table of experiments sent by Mr. Roy with his letter, and the result of the experience gained at Proctorsville, La., show conclusively, it seems to me, that although Mason's rule may hold good for an isolated pile, it cannot be depended upon for a system of piles such as are driven for foundations. In order, therefore, to determine the factor of safety for such foundations, the views and experiences of the officers of corps, it seems to me, would be valuable, and then if a proper system of experiments could be made by such of the officers as have facilities for doing so, it might lead to practical results in solving this very important question.

On September 21, 1881, Major George H. Elliot wrote me a private letter on this subject. He can undoubtedly furnish you a copy of it. It is very interesting, and the conclusions which he arrives at, seem to me very practical.

I also asked a brief opinion of Lieutenant Colonel C. B. Comstock on the general subject of pile driving, without mentioning to him the special case which produced my original letter. He has authorized me to use his reply. It is as follows:

"The energy with which a ram strikes the head of a pile is spent in changing the form of the pile, of the ram, in heating them and making them vibrate, and in most cases mainly in overcoming the friction of the earth against the pile, and in moving the particles of the earth among themselves, thus causing further friction.

"The formulæ only consider the resistance during the very short period of the blow. It would be strange if such resistance were always, for all soils, the same as when, sometime after the pile had been driven, it was loaded until it began to move. Possibly the latter resistance is sometimes the greater, usually it is doubtless much less, for most materials require a less force to change their form slowly than rapidly. A substance like clay, that is plastic, might resist driving piles very strongly and yet furnish a very much smaller resistance to a permanent load. Not knowing the relation of the two resistances, a formula which does not include that relation (*i. e.*, the character of the soil), may be, even for isolated piles, much in error. The only way to get a reliable formula seems to be to determine for characteristic, well defined, and carefully described soils, the ratio between the resistances given by some good formula like Rankine's, and the actual load, which will start the pile very slowly down and keep it going.

"In soft material a certain load spread over the surface will carry the whole of it down bodily to considerable depths. As soon as a sufficient number of piles in this area are driven and loaded, they will do the same, and additional piles are useless. In such a case the economical intervals for piles could only be found by experience."

I submit herewith Mr. Roy's table of experiments:

A TABLE OF EXPERIMENTS ON THE COMPRESSIBILITY OF SOIL OF NEW ORLEANS, LA., MADE BY MR. JOHN ROY, IN THE YEARS 1851 AND 1852.

Experiment.	No. bearings.	Size of bearing, in square inches.	Weight in pounds, applied.	Weight to the square inch, in pounds.	Sinkage in inches.	No. days to each experiment.	Depth of boring of trench, in inches.	Place of experiment, distance from the river in yards.
1	1	1/4 × 1/4 = 1/16	6.375	102.000	3 1/2	30	12	1760
2	1	1/2 × 1/2 = 1/4	25.500	102.000	7	30	12	1760
3	1	3/4 × 3/4 = 9/16	57.375	102.000	11	30	12	1760
4	1	1 × 1 = 1	102.000	102.000	11	30	12	1760
5	1	1 × 1 = 1	102.000	102.000	11	30	12	1760
6	1	1 × 2 7/8 = 2 7/8	293.250	102.000	26 3/4	30	12	1760
7	1	4 × 4 = 16	1632.000	102.000	78	30	12	1760
8	1	1 × 16 = 16	1632.000	102.000	33	30	12	1760
9	1	4 × 4 = 16	1632.000	102.000	120	161	48	1760
10	1	1/4 × 1/4 = 1/16	1.125	18.000	3/8	3	12	1760
11	1	1/4 × 1 = 1/4	4.500	18.000	5/8	3	12	1760
12	1	1/2 × 1 = 1/2	9.000	18.000	5/8	3	12	1760
13	1	3/4 × 1 = 3/4	13.500	18.000	5/8	3	12	1760
14	1	1 × 1 = 1	18.000	18.000	5/8	3	12	1760
15	1	1 × 1 = 1	36.000	36.000	2 1/2	51	12	1760
16	1	3/4 × 1 = 3/4	27.000	36.000	1 3/4	51	12	1760
17	1	1/2 × 1 = 1/2	18.000	36.000	1 1/4	51	12	1760
18	2	2 1/2 × 8 = 40	642.000	16.050	1/8	99	6	1760
19	4	1 × 1 = 4	170.000	42.500	1/8	42	0	1760
20	2	6 × 12 = 144	2552.000	17.720	1/8	107	0	400
21	2	6 × 12 = 144	3362.400	23.350	1/16	182	0	400
22	2	6 × 24 = 288	15580.000	54.097	1	48	0	300
23	1	20 7/8 × 20 7/8 = 432	18703.000	43.300	4 1/2	26	96	400
24	1	12 × 12 = 144	5132.000	35.640	3	20	96	400
25	1	24 × 24 = 576	23150.000	40.200	6 1/4	38	36	300
26	1	Weight increased.	45724.000	79.380	13 1/4	40	36	300
27	1	Weight increased.	57600.000	100.000	18 1/2	55	36	300
28	1	1 × 1 = 1	102.000	102.000	6	68	48	333
29	1	Weight increased.	202.000	202.000	18	121	48	333
30	1	4 × 4 = 16	1632.000	102.000	16 1/2	68	48	333
31	1	Weight increased.	3232.000	202.000	54 1/2	121	48	333
32	1	1 × 1 = 1	102.000	102.000	1	49	48	300
33	1	Weight increased.	202.000	202.000	7	87	48	300
34	1	4 × 4 = 16	1632.000	102.000	7	51	48	300
35	1	Weight increased.	3232.000	202.000	61 1/2	87	48	300

NOTES.—Nos. 23 and 24 were made at the new Custom House, by a Commission of U. S. Engineers, appointed by the Treasury Department.

It will be seen, by the above table, that, contrary to the general opinion, a larger surface sinks more than in proportion to its area.

A very interesting article on this subject appears in the number of VAN NOSTRAND'S ENGINEERING MAGAZINE for October, 1881. It is entitled "Note on the Friction of Timber Piles in Clay," by Arthur Cameron Hertzig, Assoc. M. Inst. C.E.

Major George H. Elliot to General Weitzel: Your letter of the 4th of August to the Chief of Engineers, relating your experience in the foundation of the Martello tower at Proctorsville, La., has suggested a comparison of the pile driving formulæ accessible to me.

Assuming in these formulæ, the case of the test pile at Proctorsville, which was thirty (30) feet long, twelve (12) by twelve and one-half (12½) inches at top, eleven (11) by eleven and one-half (11½) inches at botton; which weighed sixteen hundred and eleven (1611) pounds, and was driven by a ram weighing nine hundred and ten (910) pounds, falling five (5) feet at the last blow; the last blow driving the pile three-eighths (⅜) of an inch, the discrepancies between the results are remarkable. The extreme supporting power of this pile,

obtained from some of these formulæ, is as follows:

	Pounds.		Pounds.
Nystrom.....	17,971	Trautwine....	58,802
Mason.....	52,556	Rankine*.....128,509	
Weisbach.....	52,556		

Major Sander's formula does not give the extreme supporting power of the pile, but the safe load only—in this case, 18,200 pounds. McAlpine's formula in this case gives a negative result, as it always does when $W+228\sqrt{F}$ is less than 1, W representing the weight of the ram in tons, and F its fall in feet.

Assuming another case, a case in which the weight and fall of the ram are much greater, the discrepancies are still more remarkable. Say that the pile is of the same size and weight as the one at Proctorsville; that it makes the same penetration at the last blow, and is driven by a two thousand (2000) pound ram, falling twenty five (25) feet. The extreme supporting power and safe load in this case, according to the various authorities, are stated in the following table, in which, you will observe, the relative positions of the names of these authorities are not the same as in the preceding table.

Names of authors of formulæ and rules.	Extreme supporting power of the pile in pounds.	Safe load in pounds.
McAlpine (1).....	185,069	61,689
Trautwine (2).....	219,117	73,079
Hodgkinson (3).....	403,450	40,345
Nystrom (4).....	490,824	81,804
Rankine (5)	810.000 (6)	81,000
Do. (7).....	851.200	130,954
Mason (8)	886.080	221, 20
Weisbach (9)	886.080	48,739
The Dutch Engineers (10)	886,080	110,760
Sevelly (11)..... ...	886,080	....
Sanders (12).....	....	200,000
Haswell (13).....	....	2 0,000
Rondelet (14).....	....	69,375
Perronet (15).....	....	125,802
Rankine (16).....	....	150,000
Mahan (17)	....	150,000
Wheeler (18).....	....	150,000
Rankine (19).....	....	30,000
Mahan (20).....	....	3 ,000
Wheeler (21).....	....	30,000

*Assuming the modulus of elasticity to be 750 tons.

(1) McAlpine's formula is $P=80\,(W+.228\sqrt{F}-1)$, in which P represents the extreme supporting power of the pile in tons, W the weight of the ram in tons, and F its fall in feet. (Journal of the Franklin Institute, 3d series, Vol. LV.). His co-efficient of safety is $\frac{1}{3}$.

(2) Trautwine's formula is $P=\frac{\sqrt[3]{F}\times W\times .023}{p+1}$, in which P and F are the same as in McAlpine's formula; W the weight of the ram in pounds, and p, the penetration at the last blow, in inches. His co-efficients of safety are from $\frac{1}{3}$ to $\frac{1}{4}$, "according to circumstances." In this case and in similar cases, I have assumed the arithmetical mean. In this case, $\frac{1}{4}$.

(3) This case supposes that the pile is driven to the bed rock through soft mud, and is not supported at the sides. I have assumed in Hodgkinson's rule (Mahan's Civil Engineering, p. 50), $\frac{1}{10}$ as a co-efficient of safety.

(4) Nystrom's formula is $P=\frac{W^2F}{p(W\times w)^2}$, in which P represents the extreme supporting power of the pile in pounds; W the weight of the ram, and w the weight of the pile—both in pounds; F the fall of the ram, and p the penetration at the last blow. His co-efficient of safety is $\frac{1}{6}$.

(5) Rankine has a *rule* that "the factor of safety against direct crushing of the timber should not be less than 10."

(6) Resistance of the pile to crushing.

(7) Assuming in his *formula* the modulus of elasticity to be 750 tons. His formula is $P=\sqrt{\frac{4WFes}{l}+\frac{4e^2s^2p^2}{l^2}}-\frac{2esp}{l}$ in which P represents the extreme supporting power of the pile in tons; W the weight of the ram, and e the modulus of elasticity, both in tons; F the fall of the ram, l the length of the pile, and p the penetration at the last blow, all in feet, and s the average section of the pile in square inches. His factors of safety for use with his formula are "from 3 to 10."

(8) Colonel Mason's formula is $P=\frac{W^2}{W+w}\times\frac{F}{p}$, in which P represents the extreme supporting power of the pile; W the weight of the ram; w the weight of the pile; F the fall of the ram; and p the penetration at the last blow. His factor of safety at Fort Montgomery was 4.

(9) Weisbach's formula is the same as Mason's. His co-efficients "for duration with security" are from $\frac{1}{100}$ to $\frac{1}{10}$, the arithmetical mean if which is $\frac{1}{18\frac{1}{18}}$.

(10) Quoted in Proceedings of the Institution of Civil Engineers (British), Vol. LXIV. Their formula is the same as Mason's. Their factors of safety are from 6 to 10. I have assumed the arithmetical mean of these to find the mean co-efficient of safety.

These discrepancies show that some of these formulæ, or, at least, some of their factors of safety* are misleading, and it seems to me that all of them which have not been based upon experiments on the capacity of soils to sustain pressures, must be so.

Let us see what supports a loaded pile.

I conceive that there is below the bottom of the pile in ordinary soils a conoidal mass of earth, *a*, *b*, *c*, *d*, (Fig. 1,) the particles of which are acted upon by pressures derived from the weight of the pile and its load, and the form and dimensions of which depend on this weight and on the kind of soil;† that at every section *e*, *f*; *e*, *f*, of the pile below the surface of the ground, the particles of earth in contact with the pile, are, by reason of friction, pressed downward, and that these pressures are distributed (spread) in the same way that the pressure at the foot of the pile is distributed; that is, through the particles of the earth surrounding the pile, which are limited by conoidal surfaces, of which, (in homogeneous soils), the pile is a common axis.‡

Are the particles of earth, within these conoids of pressure and distant from the pile, acted upon by the blows of the ram?

General Tower, in remarking upon a recent device by a citizen of Virginia, for an armor protection of fortifications, consisting of a thin iron or steel plate backed by springs, said that even if the plate were one foot thick, suspended by chains, and without any backing whatever, it would be penetrated by a shot from an 81-ton gun in about $\frac{1}{1400}$ of a second, and before the plate could move perceptibly.

Is it not probable, reasoning from analogy, that the blows of the ram upon the head of a pile reach only the particles of earth which are in contact with or very near the foot and the sides of the pile; that the action (occupying only a small fraction of a second) is too quick to be communicated to more distant particles composing the conoids of pressure, and that subsequently the forces which hold these particles in place may be disturbed, and the particles may yield, under continued pressures communicated successively through the pile, and the particles of earth in contact with and near the pile?

It might appear at first sight, that if pressures are more disturbed laterally in the earth below and around a pile, the resistance to pressures must be greater than the resistance to blows, but the

It may be a question in this case, whether the mean co-efficient of safety should be $\frac{1}{3} \times \frac{1}{12}$ or $\frac{1}{4} \cdot \frac{1}{12}$ is the *geometrical* mean of $\frac{1}{3}$ and $\frac{1}{6}$, which are the co-efficients of safety corresponding to the extreme factors of safety, and it was used by the Engineer of the Portsmouth (England) Docks, as a mean co-efficient, to find the safe value of P for the piles of his work, from the formula and factors of safety of the Dutch Engineers. A similar doubt arises in finding a mean co-efficient of safety from Rankine's factors of safety.

(11) Quoted in Thomas Stevenson's "Design and Construction of Harbours." His formula is the same as Mason's. No factor of safety is given.

(12) The extreme supporting power of a pile is not given in the formula of Major Sanders, which he contributed to the Journal of the Franklin Institute, and which may be found in Vol. XXII., (3rd Series). The formula is $P = \frac{WF}{8p}$, in which P represents the safe load of the pile; F the fall of the ram; and p the penetration at the last blow.

(13) Major Sanders' formula adopted by Haswell.

(14) 427 to 498 pounds to the square inch of head of pile. Quoted in Professor Vose's "Manual for Railroad Engineers."

(15) From his rule found in *Œuvres de Perronet*. "*Nous estimons pour ces raisons, que l'on ne doit point charger les pilots de 8 à 9 pouces de grosseur, de plus de cinquante milliers; ceux d'un pied, de plus de cent milliers; et ainsi des autres à proportion du quarré de leur diametre ou de la superficie de leur tête.*"

1 millier = 1079.22 pounds. 1 pied = 12.8″

(16) 1000 pounds to the square inch of head of pile.

(17) The same.

(18) The same.

(19) "Piles standing in soft ground by friction."

(20) "Piles which resist only in virtue of the friction arising from the compression of the soil."

(21) "When they resist wholly by friction on the sides."

* By the term "factor of safety," which is used by many of the authorities on foundations, is meant the number which is to be multiplied into the working load, in any case, to find the "extreme supporting power" of the pile, or the resistance of the soil, to which, for safety in that case, the pile is to be driven.

The term "co-efficient" of safety is used by McAlpine. It is a fraction which is to be multiplied into the "extreme supporting power" of the pile to find its safe load. It is the reciprocal of the corresponding "factor of safety.

† None of the books available for reference throw any light on this subject. Rankine has a theory concerning the pressures within an earthen mass derived from its own weight, but he gives no results of experiments if any have been made, touching the action of earth under exterior pressures.

‡ In sticky soils, no doubt, the action of the particles of earth adjoining a pile, is, in part, one of drawing or pulling downward the particles of earth exterior to them, and the distance to which this action extends, depends on the degree of adhesion of these particles.

truth is, that it cannot be said that one is greater or less than the other, except by empirical comparisons between the effects of blows and the results of pressures.

When these comparisons in the case of any kind of soil have been made, the true relation between these effects and these results may be discovered, and correct and reliable factors of safety for use with formulæ for the sustaining power of piles, into which formulæ enter the terms common to all pile-driving formulæ, (viz., the weight of the ram, its fall and the average penetration of the last blows), may be made for that kind of soil, but I think it evident that no pile-driving formula or factors of safety based only on theoretical deductions from the formula $Ps=\frac{Mv^2}{2}$, can be relied on, even for single isolated piles, or for piles driven at considerable distances apart.

Now, let us examine the case of an ordinary pile foundation in any compressible soil. Say that the piles are driven three (3) feet apart, in rows the same distance apart, from center to center.

Would a safe load for this foundation be equal to the safe load of a single isolated pile in that soil, multiplied by the number of piles?

I think not, for, if it be true that below and surrounding the piles, there exists within the soil the conoids of pressure before alluded to, and if the surfaces of these conoids make any considerable angle with the vertical, then the pressure upon the earth below and between the piles, may be much greater in the case supposed, than in the case of an isolated pile.

Let Fig. 2 represent a plan of the piles of this foundation, and let Fig. 3 represect a section through one of the rows. Let *a*, *b*, *c*, *d*, Fig. 3, represent a section through the axis of the conoid of pressure arising from the pressure of the pile and its load, at the foot of the pile A, and let *a'*, *b'*, *c'*, *d'*, represent a similar section through the conoid of pressure at the foot of the pile B. Let us pass a horizontal plane at any short distance—say eighteen (18) inches—below the feet of the piles (which we suppose to be driven to a uniform depth), and let *i*, *i*, *i*, *i*, and *k*, *k*, *k*, *k*, Fig. 2, represent in plan, and let *m*, *n*, and *m' n'*, represent in section, the areas cut from the conoids of pressure by this plane, and it will be seen that considerable portions of each of these areas, may be acted upon by pressures derived from both of the piles and their loads. The same may be said of the earth within the conoids of pressure surrounding the piles, and it appears, therefore, that the forces acting upon the particles of earth below and surrounding a pile, may be in equilibrium, and the particles may be at rest, in the case of a loaded isolated pile, when the equilibrium may be disturbed, and the particles may sink with the pile, when the same load per pile is laid upon a foundation composed of piles driven in the same soil at such distances apart that their conoids of pressure intersect each other.

McAlpine, before constructing the Brooklyn Dry Dock, made experiments with loads upon piles,* and of his formula he says:

"The co-efficient is reliable for such material as was found at that place."

This material was "a silicious sand mixed with comminuted particles of mica and a little vegetable loam, and was generally encountered in the form of quicksand."

McAlpine also says:

"It is very desirable that similar experiments should be made in soils of different kinds, which would make this formula applicable to all the cases usually met with in constructions."

Major Sanders experimented by loading sets of piles of four each, and Colonel Mason made his formula when the fort (Montgomery) which he was constructing on a pile foundation, had been nearly completed.

Which of the other pile-driving formulæ and factors of safety given by the authorities I have quoted, were deduced from experiments in loading more than single isolated piles, I do not know, but some of the formulæ appear to have been based only on theoretical considerations, and some of the factors of safety appear to be simply conjectural.

None of the formulæ are accompanied

* As far as I can determine from his paper read before the Franklin Institute, January 15, 1868, these experiments were made (by means of a lever), upon isolated piles only.

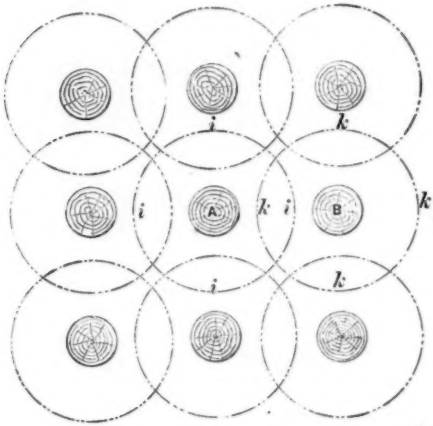


Fig.2.

Fig.1.

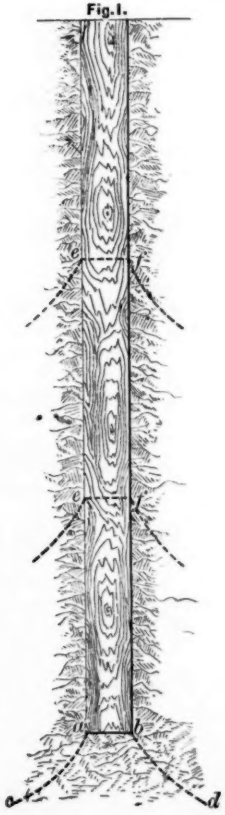
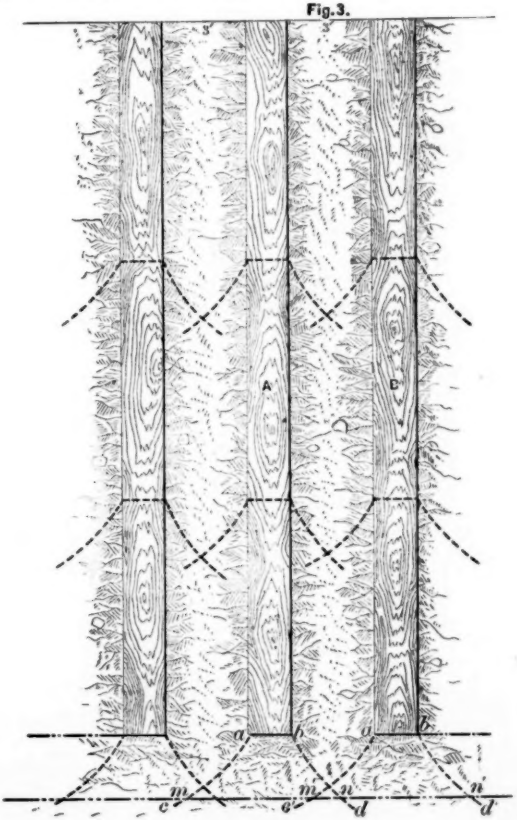


Fig.3.



by tables of factors of safety, corresponding to specified kinds of soil.

It is factors of safety that are most needed. There are many formulæ. Doubtless most of them are good, and one of them—$P=\frac{W^2}{W+w}\times\frac{F}{p}$,—has been deduced independently by several distinguished authors; but can any of them be used safely and confidently, when the factors of safety furnished by the authors of these formulæ produce results so discordant?

An engineer having to construct a pile foundation, must take some pile-driving formula and factor of safety, as he finds them. He has no time to make proper experiments in the soil he has to deal with, for that would require years of time.

It is not enough for his purpose that an author of a formula prescribes for use with it, a single factor of safety of 3, for example, for he knows that that factor can only be a proper one for one kind of soil, and he is not told what the kind of soil is. It may be more, or it may be less easily penetrated than his own. In the former case, by the use of an unnecessarily large factor of safety, he would make his foundation unnecessarily expensive; and in the latter, his foundation would be in danger of yielding, sometime, under its load. Neither is he satisfied to be told to use a factor of safety from 3 to 10; from 6 to 10, or from 10 to 100, "according to circumstances." He wants his own case and its proper factor of safety to be, as far as possible, definitely stated, or else, it seems to me, he would prefer to drive the piles of his foundation in every case of importance, as far as they will go, or to the equivalent of their "absolute stoppage,"* which, he knows, would make his foundation as safe as a pile foundation can be made, though it may be expensive.

I think that the want of reliable and definite factors of safety can, in a manner, be supplied, without waiting for experiments made for the purpose.

While it is difficult, no doubt, to make minute descriptions of soils by giving the proportions of their physical constituents, I think that a table of useful factors of safety, corresponding to quite a large number of the ordinary and easily recognizable soils, could be made for use with any good formula, say Mason's, from the past recorded experiences of the officers of the Corps of Engineers. This could be done by dividing the values of P deduced from that formula, (substituting in each case for W, F, w, and p, the actual weight and fall of the ram, the average weight of the piles, and the average penetration at the last blows) by the actual weights of the structures per pile.

A comparison of all the factors of safety, obtained in this way, which would arise from cases in which foundations in any specified kind of soil have carried their loads for some years without any evidence of settling, would probably show that no two of them would be precisely the same, and that some of them would be excessive. These latter, which would lead to unnecessarily expensive work, and any inadequate factor which might be developed by a failure of a foundation, like the one at Proctorsville, to carry its load, could be rejected. A fair judgment could then be taken in respect of the others, and a single safe and reliable factor for that kind of soil, could be determined on.

From the foregoing considerations, I come to the following conclusions:

1st. Pile-driving formulæ should be accompanied by tables of factors of safety, corresponding to all the common and easily recognizible kinds of soil.

2nd. These factors of safety should be determined on after extended experiments on the supporting power of piles,* although approximate factors which could be used without hazard, could be found from examinations of the records of the driving of the piles of actual foundations, provided the weights of the superstructures are known, and descriptions of the soils have been preserved; and provided, also, that the foundations have carried their loads during sufficient lengths of time.

* p=.0067″ when W=800 pounds and F=5′. See Mahan's Civil Engineering. It is the *refus du mouton* described in *Œuvres de Perronet*. By Mason's formula, it appears that this equivalent would be reached when seven 7) blows from a two thousand 2,000) pound ram, falling twenty-five 25) feet, would sink a sixteen hundred and eleven (1611) pound pile one (1) inch.

* The case mentioned by you shows that the testing by loading should extend over considerable lengths of time. Even the foundations of Fort Montgomery and Fort Delaware have settled more or less.

3rd. In experiments on the supporting power of piles the loads should not rest upon single isolated piles, but they should cover a number of piles, driven at those distances apart which are usual in pile foundations.

4th. In every case of construction of a pile foundation, the record of the driving of the piles, should include such a description of the soil, obtained for borings, as would enable an engineer, having to found a work in a similar soil, to recognise it.

EXPERIMENTAL PROOFS OF SOME NEW FORMULÆ FOR THE TORSION OF PRISMATIC BODIES.

By PROF. J. BAUSHINGER.

From "Der Civilingenieur," for Abstracts of the Institution of Civil Engineers.

THE author commences with nearly a column of explanation of the symbols used, and then applying his formulæ to five bars of the following sections: (1) circular; (2)elliptical, with axes in ratio of 1 : 2; (3)square; (4) rectangular, with sides in ratio of 1:2; (5) rectangular, with sides as 1 : 4, he deduces the following equation:—

$$d_1 : d_2 : d_3 : d_4 : d_5 = 1 : 1.25 : 1.13 : 1.40 : 9.1,$$

where d_1 is the amount of rotation which a cross section of the circular bar takes relatively to a parallel one at a fixed distance from it under the action of a given force; d_2 is the corresponding amount in the bar of elliptic section under the same force, and so on.

It should be noticed that the dimensions of the bars are so adjusted that the areas of Nos. 1, 2. 3 and 4 are equal to each other, and the area of No. 5 (sides as 1 : 4) half either of the others.

By an approximate formula the above quotation becomes=

$$d_1 : d_2 : d_3 : d_4 : d_5 = 1 : 1.25 : 1.05 : 1.31 : 8.9.$$

Experimental results were obtained as follows:—Five pairs of bars of cast iron each 100 centimeters long and of the above sections were twisted in a Werder's testing machine as explained in the author's already published *Essais de Resistance*. The cross sections, the relative rotations of which were measured, were 50 centimeters apart, and the rotation was measured on the arc of a circle of 350 centimeters radius (or rather on the tangent to such a circle) by means of telescopes, special precautions being taken to eliminate errors and secure exact readings. Tables of results are given, from which it appears that taking the circular bar as the standard of comparison, experiment agrees well with theory in the case of the bar of elliptic section; but the agreement is not so close as could be desired with the square and rectangular bars. With them the observed rotations are greater than the values given by the first of the above equations, and harmonize still less with those of the approximate equation, which are smaller than those obtained from the rigorous formula.

Reference is made in the paper to experiments on torsion, the particulars of which are given in tables 122 to 147 of the *Essais de Resistance* already referred to. These experiments were made on bars of Siemens Martin steel of various degrees of hardness, of Bessemer steel similarly varying, and of iron both granular and fibrous in texture. The bars were 660 millimeters long, and circular or square in section, the diameter or side being in each case 10 centimeters. By the formula the relative amount of rotation of two bars of the same material should be given by

$$d_1 : d_2 :: 1 : 0.698,$$

and though there is some discrepancy between the experimental and theoretical results in individual cases, yet the average of thirteen pairs of bars gives

$$d_1 : d_2 :: 1 : 0.696.$$

The thirteen values range between

1 : 0.633 in iron bars of fine grain,

and

1 : 0.747 in Bessemer steel bars.

A further proof of the formulæ is obtained by deducing from them the modulus of shearing elasticity (η), and comparing the results with those obtained from the formula,

$$\eta = \frac{\varepsilon}{2(1+\mu)},$$

where ε is the modulus of tensile or compressive elasticity, and μ is the ratio between the sectional contraction or dilatation, and the increase or diminution of length produced by direct tensile or compressive stresses. Tables of values are given, and they agree as well as could be expected when the minute quantities to be measured are considered, and it is worthy of notice that the ratio μ is practically independent of the form of the cross section.

A formula given by the author for the maximum sheering stress produced in a section by torsion, cannot be proved directly, since it is impossible to measure the stress at any precise spot. The method adopted was to increase the moment of torsion till rupture ensued, and to compare the corresponding values of maximum stress as given by the formula (which may be called the "strength of torsion") (*torsions festigkeit*), in the case of bars of different sections. As might be expected, the form of the cross section had in this case very great influence on the result; the section of greatest strength being the circular, and next to it the square, the least favorable being the rectangular with sides as 1 : 4. The proportional figures for the maximum stress produced by an equal moment of torsion were

1 : 1.414 : 1.269 : 1.795 : 2.539,

the order of the bars being that previously given.

The author proposes to make further experiments on the torsion of bars of similar sections but of varying dimensions.

Application of the Radiophone to Telegraphy.—By E. Mecadier.—The author causes each radiophonic transmitter to induce vibrations in the electric circuit corresponding to a definite musical tone, and by intermitting the rays of light falling on the perforated revolving disc, by a disc attached to a Morse key, obtains in each receiving telephone Morse signals in musical tones. By instructing each operator to distinguish only those signals corresponding to a given tone, it is found possible to transmit numerous messages in either direction at one and the same time. The selenium cells of the radiophones and the telephones are all included in a single direct circuit.—*Comptes rendus de l'Academie des Sciences.*

Electrical Thermometers for Observing Temperature at a Distance.—By Max Lindner.—In 1877 Herr Eichhorn made experiments with several platinum wires hermetically sealed into the sides of a thermometer, at such distances that a rough graduation was possible by the electrical contact made by the rising or falling mercury; and in this year he used the instrument in a malt manufactory, with much success, for the regulation of the heating arrangements.

For use in brewing, the firm of Oscar Schoppe, of Leipsic, enclose the thermometer in a wooden case, and they can connect the several wires at will with electro magnetic bell arrangements, so that a bell rings as soon as the temperature reaches a certain height. The distances to which these wires have to be taken are usually small, and only a few wires are necessary, so that the cable is not of an expensive character. The insulating material of the silk-covered wires of the cable is asphalt. The temperatures of cooling vessels, as well as heating vessels, are controlled by means of these thermometers, which are also employed for opening and closing ventilators, &c. They act very well everywhere, and may be depended on, and this is in favorable comparison with the bad action of the ordinary thermo-electric thermometers. *Zeitschrift fur Angewandte Elektricitatslehre.*

CANDLE POWER OF THE ELECTRIC LIGHT.

By PAGET HIGGS, LL.D.

From Proceedings of the Institution of Civil Engineers.

I.

VERY varying statements are constantly before the public as to the candle power of diverse devices affording the electric light. None of these statements appear to be compatible, neither does any law of difference immediately present itself. Just as in a diagram of results the sanguine mathematician may picture to himself the curve representing a definite law where the unimaginative observer can perceive only a chaotic zigzag of dots, so with a little bias there, and a small subtraction here, some order may be evolved from the figures relating to the electric light. Such an attempt is made in what follows.

The most salient point for a unit of comparison is the number of heat units represented by electrical measurement, as in ratio with the candle power measured optically. But at the outset a difficulty, or rather an uncertainty, is experienced; this refers, however, only to arc-lights, of which there are two systems of measurement—one system with the carbons on the same axis, the other with the axis of one of the carbons forming a very acute angle with the axis of the other carbon, so that the glowing crater of one carbon forms a reflector to the point of the other. In the latter case, considering the light of the former as unity, the light may be about 1.66 time stronger as measured. This has been pointed out by Mr. Douglass, M. Inst. C.E., in a Report to the Trinity House. Another source of discrepancy is the want of knowledge of the specific heat of the vapor of the electric arc, and of its temperature, both unknown quantities; if the one were known, the other could be determined.

Taking the ratio of units of heat represented per candle power, the subsequent figures will show a large margin of economy for arc lighting over incandescent lighting. This will of course be true of the arc considered only as a furnace producing a greater heat in a smaller space then by incandescence; and it appears to the author to be true for another reason. Whatever may be the specific heat of the vapor of the electric arc, it is certain that over the given resistance of the arc, as compared with an equal resistance of the incandescent lamp, the mass of the arc, measured by the molecules it contains, is far less than that of the solid carbon; and the amount of work to be done by the current from this cause will be so considerably less, as to lead to a prophetic renunciation of greater economy of expended energy than is really found.

To return to figures. Suppose a light of 1000 candle power, measured with the carbons on the same axis, be produced with 4.5 ohms resistance and 10 webers of current, there will be represented 108 gramme degrees of heat, or nearly 0.1 gramme degree per candle power per second. This is deducible from the figures given by the Brush system. It does not include the heat due to consumption of carbon in air, which is inconsiderable.

In a Siemens lamp tested by the author, about 3,000 candle power, of diffused beam, was obtained with 36 webers current, when the lamp had 1 ohm of resistance in the arc; this corresponds to 335 heat units, or $\left(\frac{335}{3{,}000}\right)$ 0.112 unit per candle power. In a Serrin lamp, fed from a Gramme machine, the author obtained a light of 3,600 candle power with 45.7 webers current, the arc having $1\frac{1}{4}$ ohm resistance, corresponding to 624 heat units, or 0.17 unit per candle. A Crompton lamp, fed by a Burgin machine gave a light said to be of 4,000 candle power; but assuming this to be from bi-axial position of the carbons, about 2,000 candle power would correspond to 180 heat units for 16 webers on 2.93 ohms, or about 0.09 heat unit per candle power. On (about) the same resistance of arc in a Crompton lamp, 24 webers yielded the author 3,600 candle power, or about 403 heat units, corresponding to 0.12 heat unit per candle power.

Numerous measurements are recorded,

all varying greatly, partly and chiefly because of the variations in the measurements of candle power. All the measurements, as recorded by the author, have been made by the same method from the diffused "beam."

Their mean may therefore be taken for comparison with subsequent numbers. It is 0.118 gramme degree per candle power.

As 1 gramme degree=42 million ergs, 1 candle power represents 4.9 million ergs. As a foot pound is 13.56 million ergs, each candle power represents 0.364 foot lb. per second, or 1,511 candle power per HP., a rough check upon the foregoing figures.

The late Mr. L. Schwendler, M. Inst. C E., has stated in a Paper (fragmentary to the author) that the standard candle does work at the rate of 610 meg-ergs in a second, whilst the unit of light is produced electrically at the rate of not more than 20 meg-ergs in a second. This latter figure is very high if it refer to arc lighting, for, although at the trials under the auspices of the Franklin Institute, when only 380 candle power per HP. were obtained, there were estimated to to be ($6.5 \times 0.252 =$) 1.6 gramme degree= 67 meg-ergs per candle power, great strides have since been made. Mr. Schwendler's figures are now at a long discount, and would appear corresponding to a still lower state of the art if the figures given by others be correct as to candle power of the lights. As has been stated, however, the figures given in this Paper are intended to be only intercomparative.

Another type of lamp is the Werdermann, which may be termed an arc incandescent lamp, because the light is obtained from the incandescence of a cone of carbon resting at its apex on a negative electrode of larger section, and from the arc that plays between the sides of the carbon cone and face of the negative electrode. Ten of these lamps, giving 40 candle-power light, each burning 4.5 millimeter carbons, yielded about 0.88 heat unit per candle power. A series of these lamps averaged 306 candle power, with 50 webers current, the resistance of each lamp being 0.1337 ohm. This corresponds to 80 heat units per lamp, or to 0.262 heat unit per candle power. Thus, the small light is a sub-multiple to a considerable degree of the larger light, want of economy commences to be evident, and an average can no longer be taken.

A Joel lamp, one of a series of ten, is said to have afforded 320 candle power, with an electro-motive force of 130 volts, sending a current of 50 webers through the series, corresponding to 156 heat units per lamp, or 0.49 heat unit per candle power.

These notes, however crude, have more weight when purely incandescent lamps are considered. In this case measurement becomes easy, for the light approximates in color to that of the standard candle employed, and the resistance of the incandescent fiber is sufficiently constant to yield concordant results.

One of Maxim's earliest lamps was measured by the author, and found to indicate 3.6 ohms when cold, and 1.9 ohm when giving 11.5 candle-power light with a current of 5.5 webers. This corresponds to 0.83 unit per candle power, or about 140 candle power per HP. It should be remarked that with this current the loss due to heat per unit of resistance in the conductors would be 3 per cent. as against the 0.1 per cent. for a weber current. Another Maxim lamp of about 64 ohms when giving 50 candle power, and 116 ohms when cold, with 1.3 weber current, would correspond to 0.52 heat unit per candle power. An Edison lamp, in the author's possession, measures 61 ohms when cold and 33 ohms when hot, and indicates, with 1 weber of current, 11 candle power, equivalent to 0.73 heat unit per candle power.

A Swan lamp had not, at the time of the author's measurements, found its way to America; but there are several statements as to the candle power of this lamp. It would appear that with 160 volts and 24 webers of current, 24 rows of two lamps in series, or 48 lamps, each of 84 ohms resistance, gave 48 candle power each. Assuming that this was the resistance of the lamp when cold, that the resistance when incandescent would be 33 ohms, and that there would then be 2 webers passing through each lamp, this would correspond to 0.66 heat unit per candle power. These are, however, assumed figures.

It should be clearly understood in estimating the work done in any carbon

focus that the resistance of the carbon decreases with the increase of temperature, and that, if the current be directly taken from a dynamo machine, constructed on the mutual accumulation principle, there will be considerably more current flowing through the lamp than an estimate based on a potential measurement will allow.

The following table furnishes a comprehensive view of the results obtained. (The figures are only roughly calculated.)

A 5-feet gas-burner supplying 16 candle power light would cost for a 4-light chandelier, for 20 cubic feet of gas, in New York $\$2.50 \times .02 = \$0.05$ or 5 cents an hour. At $40 a year cost, or adding 25 per cent. for profit, at $50 a year, 1 HP. can be had for about 300 working hours a year; and $\frac{5{,}000}{300} = 6.16$ cents an hour, or $\frac{16.6}{4} = 4.15$ cents per hour for the electric chandelier. This shows that, even now, were a reasonable commercial profit taken, the electric light, in the United States at least, could compete with gas.

TABLE I.

Actual Diffused Light in Focus.	Candle Power per HP. in Focus.	Gramme Degree per Candle Power per Second.	Foot lbs. per Minute per Candle Power	Remarks.
1,000	1,774	0.10	19	Arc. Brush.
3,000	1,650	0.11	20	" Siemens, as found.
3,600	1,030	0.17	32	" Serrin.
3,600	1,500	0.12	22	" Crompton.
....	1,500	0.12	22	" (Mean.)
40	200	0.88	164	" Incandescent.
306	684	0.26	48	" Werdermann.
320	363	0.49	91	" Joel.
11½	214	0.83	154	Incandescent Maxim.
50	280	0.64	119	" "
50	345	0.52	96	" "
11	245	0.73	136	" Edison.
48	270	0.66	123	" Swan, estimated.

It is at present impossible to estimate the loss due to decrease of resistance in the carbon by expenditure of heat, but it must be considerable.

The author hopes that from this it will appear in how far the incandescent light is theoretically more costly than the arc light, as about 6 to 1. But in practical use there are other considerations, not the smallest of which is the attendance arc lights require to maintain their store of carbon.

The light employed in ordinary domestic avocations is approximately 1 candle (standard) at 1 foot distance. Assuming an average distance of 8 feet for domestic lighting, the electric chandelier must be of 64 candle power to give the same "surface intensity," in a room 16 feet square and of slightly more than ordinary height. The incandescent lamp will give this light at an expenditure of 0.6 heat unit per candle power, or 38.4 heat units per light center, or say four chandeliers per HP.

A paper by Sir William Thomson and Mr. Bottomley, entitled "The Illuminating Powers of Incandescent Vacuum Lamps, with Measured Potentials and Measured Currents," * read at the last meeting of the British Association, contains a table from which a valuable law can be deduced, a law that the author first enunciated before the Institution in 1878. It is that the light in an electric system varies as the fourth power of the current whose resistance or potential is constant, or as the second power of the work in circuit. To illustrate this, columns *a*, *b*, *c* and *d* have been taken from the tables in the paper referred to, and *e* and *f* calculated. The agreement is sufficiently close.

The value of the candle power in heat units is higher than observed by the

* *Vide* "Nature," vol. xxiv., p. 490.

author, and this is probably due to the method employed in measurement of the light, which is more wasteful of the observed rays than that used by the author.

The law just referred to is illustrated by the following table:

TABLE II.

a. Volts.	*b.* Webers.	*c.* HP.	*d.* Candles.	Observed Ratio of Light. *e.*	Estimated Ratio of Light. *f.*
56.9	1.21	0.093	11.6	1.00	1.0
65.5	1.46	0.129	25.0	2.16	1.9
70.2	1.64	0.156	42.0	3.62	2.8
74.1	1.81	0.181	44.0	3.79	3.9
76.1	1.82	0.187	55.0	4.75	4.1
78.0	1.99	0.210	63.0	5.42	5.2
80.3	2.06	0.224	66.0	5.70	5.9
81.9	2.06	0.228	76.0	6.54	6.3
84.6	2.06	0.235	82.0	7.05	6.5
87.0	2.10	0.247	84.0	7.24	7.2
90.9	2.17	0.267	102.0	8.80	8.4
99.1	2.21	0.296	114.0	9.85	9.8

Considering that in the measuring galvanomoter, although a very accurate instrument, the deflections are merely proportional to the effect, and liability of error will be small; and that in the photometer used (an inaccurate instrument) the measurements vary with the second power of the distance, whilst the light under measurement varies with the fourth power of the current, the departures from agreement of the observed and estimated figures may be fully ascribed to errors of observation.

DISCUSSION.

Mr. J. W. Swan remarked, through the Secretary, that even if the material was not as large, nor the conditions, under which the observations were made, as perfect as could have been wished, the paper at least formed an interesting contribution on a difficult and important subject. He doubted, however, whether the facts adduced were sufficient to establish, or even to strongly support, the theoretical views expressed, more particularly with regard to the comparative economy of the arc light and of the incandescent light. He failed to see why it might not be possible to obtain as large an amount of light for a given expenditure of energy invested in a series of incandescent lamps as in an arc light. It was perhaps not possible to raise the carbon filament of an incandescent lamp to quite the same degree of intense brilliance as the crater in the positive electrode of an arc lamp; but there was full compensation for the somewhat lower incandescence of the carbon filament in the large radiating surface obtained through a multiplication of such filaments. He had seen produced by incandescent lamps the light of between 2,000 and 3,000 candles by the expenditure of 1 HP. He did not say that the lamps were durable at the exceedingly high temperature to which it was necessary to heat the filaments in order to obtain this result; but that was a practical consideration, and he merely submitted the fact as bearing upon the theoretical view sought to be established by the tables. He noticed a discrepancy in the figures on which the calculation of the HP. product of light from Swan lamps was based. It was stated that there were 24 rows of lamps with two lamps in each row, that the light given by each lamp was 48 candle power, that the current was 24 webers and the potential 160 volts. The resistance of the lamps cold was mentioned, but the resistance hot was assumed, and this assumption was supposed to introduce an element of uncertainty into the calculation. But if the current and the electro-motive force were known, and both these were stated, the one as 160 volts and the other as 24 webers, that was one weber through each of the 24 lines, and therefore through each lamp—a current more likely to be correct than the 2 webers also mentioned, and which presupposed a total current of 48 webers instead of 24 given as the total; then it followed that the light per HP. was 438 candle power, and not 270, as given in the table of measurements. Probably it had been overlooked that as two lamps were in series, the 160 volts electro-motive force, and one weber current, lighted two lamps, and that the united light of the two must therefore be taken as the product of this expenditure of energy. Whether this was the correct explanation of the error or not, it was certain that with the correction he had suggested the result was much more concordant with the numerous other measurements. Referring to the remark, "that from this it will appear in how far

the incandescent light is theoretically more costly than the arc light, as about 6 to 1," he would only add, that it appeared to him that a much broader basis of observation than that supplied by the tables of measurement contained in the paper was required to support the theory sought to be erected upon it.

Mr. H. WILDE observed, through the Secretary, that in considering that part of the paper which related to incandescent lighting, the following observations might perhaps be found useful. In the various accounts and descriptions of this method of lighting which had appeared from time to time, a striking feature was the absence of any precise information as to the amount of disintegration of the carbon filament during the transmission of the electric current, and on which the durability or life of the lamp depended. The determination of this question, as would be obvious, preceded all others in order of importance, when the new method of lighting was compared with other illuminants in point of economy and convenience. From experiments which he had made, with Swan's lamps of the most recent manufacture, he had found that the carbon filament, after being maintained at the parliamentary standard of a single gas light of 16 candles, broke down in one hundred and forty to one hundred and fifty hours. In these experiments care was taken to maintain the light as nearly uniform as possible, and the comparison was made by Rumford's photometer and a standard wax candle. After the lamps had been lighted for some hours, a deposit of carbon was formed in the interior of the glass globe, which was attended by a visible diminution of the thickness of the carbon filament. This deposit increased in density sufficient to diminish the available light from the filament by 3 or 4 candle power before it broke down. The depth of coloration of the glass globe afforded a ready means of estimating, approximately, the number of hours which a lamp had been in operation at a given candle power. Further observations indicated that the durability of the carbon filaments of incandescent lamps was inversely proportional to the square of the luminous intensity. Hence, the life of a carbon which was one hundred and fifty hours at a power of 16 candles would be extended to six hundred hours at a power of 8 candles; while with a power of 32 candles the life of a carbon would be diminished to thirty-eight hours. It would therefore appear that this lamp was only practicable for light below 16 candle power.

There was no reason to expect a better duty from other incandescent lamps in which a carbon filament was used than was obtained from the Swan lamp, as the metallic lustre and ring of the filament in this lamp showed that the conversion of the hydro carbon, of which it was composed, into pure carbon, had been complete. The determination of the durability of the filament of an incandescent lamp thus afforded a basis of comparison with other methods of illumination in point of economy. Now, 750 cubic feet of standard, or 16 candle gas, were the equivalent of the life of a Swan lamp of the same illuminating power for one hundred and fifty hours, which, with gas at 3s. per 1,000 cubic feet, the price in London, amounted to 2s. 3d. for the same amount of light for one hundred and fifty hours as from a Swan lamp. In this sum was included the cost of manufacture, distribution, and profit on the gas, which was not more than the manufacturing cost of renewing the incandescent lamp alone. He left untouched the subject of the generation, distribution, and subdivision of the electricity for lighting incandescent lamps over large areas, as it was attended with so many difficulties, electrical and mechanical, that all comparison with regard to cost would be purely hypothetical; but which, even if these difficulties were overcome, would place the cost of incandescent lighting largely in excess of the cost of gas light. While viewing, as he did, the substitution of incandescent for gas light as a retrograde step in general domestic and public lighting, there were special applications of the new illuminant which were of undoubted value. The lighting of the interior of steamships by incandescent lamps had so far been attended with very promising success; but in this case considerations of cost were far outweighed by the superior advantages of comfort and convenience which the new illuminant afforded over oil lights, for which it was substituted. Other uses would without doubt be found hereafter

for incandescent lighting; and although its application might not be so universal as the promoters of it anticipated, the invention promised to be a permanent and valuable addition to the resources of artificial illumination.

Mr. H. E. JONES said, although no professed electrician, he had nevertheless been struck with what seemed to him to be two fallacies in the paper. First, the author appeared to assume that there was a distinct ratio between the heat units observed and the amount of light given. That was certainly contrary to his experience of photometric experiments with other lights. In fact, with regard to gas lights it was exactly in the inverse ratio, for the most heat from gas light was coincident with the worst illuminating power. That part of the paper, however, with which he found most fault was an error in the statements which had been made from time to time about the electric light and which in his view discredited those connected with it. An attempt was made to draw a comparison between the cost of electric light and that of gas, but in estimating the cost of the electric light the author stopped short at the HP. cost of production. In the appendix to the Report of the Electric Light Committee, June, 1879, p. 243, it was stated that of the total cost, 37.11 francs, of a certain number of lamps, something like 31 francs attached to the carbon, altogether independent of machine and HP. In the present case the author had taken the cost of gas at 2½ dollars per 1,000 cubic feet in New York, and to compare the cost of the electric light with that, there must be added expenses of distribution, management, wear and tear of machinery, and interest upon capital, which altogether was no very small item. The published accounts of a large Metropolitan Gas Company showed that the rates and taxes, the collection and the making up of the accounts in the office, the distribution expenses, cost of inspecting the lighting, and so on, came to three quarters of the net cost of material for the gas, deducting the product received from the coal used. When the advocates of the electric light had obtained a business, which they had not at present, they would be confronted with these expenses; they would also be confronted with the dividend payable to their shareholders, which would have to be met by a balance at the bank, and not by bills and promissory notes, paid for the assumed privilege of lighting some other part of England with a light which, as shown in London, made outsiders think that it was a commercial success. It had been shown in the streets of London; the misguided foreigner came over and thought that the city was being lighted in competition with gas in the most successful manner; the figures of cost were kept out of sight; and the foreigner went and bought a concession of some patent for electric lighting. That was a profitable operation. He did not wish to wander from the precise subject, but he spoke essentially as a gas engineer. It was said when the electric light was first brought into London that there would be seen on the Embankment lights of 1,000 candle power, but what was the result? It was found, when tested with the photometer by Mr. Keates,* that the light was only 150 candle power. If any gentleman drove over London bridge on a dark night he would find the passage a difficult one; he had made it constantly for the purpose of observing the electric lighting, and the conclusion in his mind was that the lighting of some parts of the city now, practically by the Electric Light Companies, was a ghastly failure. That it was a very extravagant one was proved by a document printed by the Common Council, showing the tenders for electric lighting in the City of London, and proving that it was costing for current expenses three or four times as much as gas; and when the expenses of wear and tear, and so forth, were added, it would be seen what a costly thing electric light was. The author appeared to have written the paper for the purpose of bolstering up the electric light at the expense of gas, and claimed for it that which Mr. Jones did not hesitate to say, and which every one practically acquainted with the carrying on of a commercial undertaking on a very large scale would know, was only a fraction of the cost, viz., the HP. of developing the light. No confidence could be reposed in such a comparison. There should have been added the carbons, the wear

* *Vide* Report to Metropolitan Board of Works, May, 1879, p. 11.

and tear of the machines, which were running eight hundred revolutions per minute, the original cost of the plant, the depreciation, which, with machinery running at that speed, was 15 to 20 per cent. per annum, and also the managerial and general expenses, which, as shown in the case he had quoted of a Metropolitan Company, where the rates and taxes alone amounted to 30 per cent. of the net cost of the gas for coals, after deducting the value of the products. One other point he wished to notice was this; a great deal had been said of what light could be developed from 1 lb. of coal burnt on the bars of a steam engine developing electric light, and it was assumed that that was something enormous compared with what the gas engineer made of it. Now he wished to say that 1 lb. of coal could not be treated more economically than by the gas engineer. He took it, distilled it analytically, brought out the fixed, gaseous, and liquid carbons, and then returned a fuel out of the coal which was essentially the fuel of the poor; and besides that, he got the light, and many other things. There had also now been obtained something approaching to a good gas engine, and it had been found that gas used in that way was really more effective than the coal burnt under the boiler. Therefore all the exaggerated contempt that was poured by ignorant people upon gas, as contrasted with the electric light, was very much misplaced. There was much ignorance abroad; he was guilty of it himself to some extent with regard to electricity. As he had frequently replied to people when they had asked him upon the subject, electricity, as applied to lighting and to power, was analogous to water which was pumped into an accumulator under pressure, and liberated through the crane or other machine, being a transmitter of energy and not an original power, which could be gathered anywhere, and turned at once to the service of man. He would like to direct the attention of the members to the article on the subject of the cost of Electric Light in *The Engineer* of the 13th of January, 1882.

Mr. R. E. Crompton observed that it had been pointed out how engineers could obtain a cheap source of power by using the gas engine, and their attention had been called to the point, that with the primary object of supplying the public with light, by means of gas, the manufacturers obtained secondary products of importance, quite equal to, in fact, almost greater than the gas itself. He thanked Mr. Jones for this; in future electric light engineers would be able to obtain all the useful residual products from their lb. of coal by the ordinary process of distillation, and simply use the gas as a means of obtaining motive power for producing the electric current. He had, however, prepared a few notes on a different part of the subject, namely, the purely scientific question of the candle power of the electric light. He noticed that almost at the commencement the author confessed that but little was known of the specific heat of the vapor of the electric arc and of its temperature. This admission had greatly disappointed him, as from his own observations he had long since formed an opinion that the candle power of the electric light, whether the arc light or the incandescent light, was a function of, or at all events closely allied to, its temperature, and from the title of this paper he fully hoped for some information on the point. In incandescent lamps the relation of temperature to lighting power was self-evident, as the temperatures were comparatively low, and the changes in color, marking the changes in temperature, could be followed by the eye. But with the arc light it was different. The greater intensity of the light made it difficult, and almost dangerous, to observe it closely, and it was only by the use of the spectroscope, or by similar means, that changes of these exalted temperatures could be observed. The author had unnecessarily complicated the matter by introducing the regulating arc lamps themselves. They occupied but a secondary part in obtaining high efficiency in candle power from a given electric current. So long as they held the carbons firmly in line, and fed them together with due regularity, so as to maintain a constant difference of potential on the two sides of the arc, they did all they could towards this efficiency. What had mainly to be looked to was the obtaining of a higher temperature at the arc, and this by perfecting the carbon rods. The carbon rods must excel in

City of of London—Electric Lighting, 1880.

Abstract of tenders received by the Streets Committee of the Commissioners of Sewers on the 28th day of October, 1880, for lighting the thoroughfares of New Bridge Street, Ludgate Circus, Ludgate Hill, St. Paul's Churchyard (North side), Cheapside, Poultry, Mansion House Street, Royal Exchange (open space in front of), King William Street, Adelaide Place, Queen Street, Queen Street Place, Queen Victoria Street, King Street, Guildhall Yard, London Bridge, Southwark Bridge, and Blackfriars Bridge.

District No. 1.—Comprising Blackfriars Bridge, New Bridge Street, Ludgate Circus, Ludgate Hill, St. Paul's Churchyard (North side), and Cheapside (from Western end to King Street):—

Name of Contractor Tendering.	To light for 12 months, from Sunset to Sunrise.	To provide and fix Machinery, Lamps, &c., and remove same at expiration of Contract.	Total Cost of 12 Months' Trial.	Number of Electric Lamps to be Lighted.	Number of Gas Lamps not to be Lighted when Electric Lamps are alight.	
	£	£	£		£	
Anglo-American Electric Light Company ("Brush" System).	660 abt. (same price as Commission pays for gas.)	750	1,410	32	150 abt.	=600
Crompton & Co.....	2,007	500	2,507	17	152	=608
Electric and Magnetic Company ("Jablochkoff" System.).....	1,500	1,550*	3,050	48	144	=576
Siemens Brothers.....	2,050	1,650	3,700	29 (viz., 23 small, 6 large.)	144	=576

District No. 2.—Comprising Southwark Bridge, Queen Victoria Street, Queen Street (between Queen Victoria Street and Upper Thames Street), and Queen Street Place:—

Anglo-American Electric Light Company ("Brush" System).	....	No tender.				
Crompton & Co.....	2,167	560	2,727	16	176	=704
Electric and Magnetic Company ("Jablochkoff" System.).....	1,580	1,350*	2,930	52	161	=644
Siemens Brothers.....	1,850	980	2,830	31 (viz., 26 small, 5 large.)	164	=656

District No. 3.—Comprising London Bridge, Queen Street (between Queen Victoria Street and Cheapside), Cheapside (between King Street and Poultry), King Street, Guildhall Yard, Poultry, Mansion House Street, Royal Exchange (open space in front of), King William Street, and Adelaide Place:—

Anglo-American Electric Light Company ("Brush" System).	....	No tender.				
Crompton & Co.....	2,475	650	3,125	18	132	=528
Electric and Magnetic Company ("Jablochkoff" System.).....	....	No tender.				
Siemens Brothers.....	2,270	1,450	3,720	32 (viv., 26 small, 6 large.)	138	=552

* Should the Commission determine to have the conductors laid underground, the additional cost for each district will be £2,000 and £2,000 more for removing them and making good after.

N. B.—The black figures are not in original, but represent about the cost of the gas lighting.

two main points; first they must be extremely refractory and infusible, in other words, be pure, and free from even the smallest percentage of material more easily volatilizable than the carbon itself. Secondly, they must be hard, dense and compact, so as to oppose as much resistance to the disintegrating action of the current as possible, thus necessitating the much desired extreme temperatures. The wide discrepancies noticed between different photometric measurements of the same electric light system were mainly due to the differences in purity and density of the carbons. Pure carbons of little density, or dense carbons containing considerable impurity, were equally adverse to high candle power. Carbons had been moulded from absolutely pure carbon, yet of loose texture, which would not afford anything more than a pale blue light of 50 or 60 candles, when a 20 ampère current was used, and almost equally bad results had been given by well-made dense rods, containing not more than 5 per cent. of lime, soda and other ash. Moreover, the same rods varied considerably from inch to inch, and this would often account for the great changes in brilliancy observable in the arc lights in public use. The blame for the variation in the light was generally visited on the lamps, machines or engine, but now-a-days the blame ought to rest far oftener on the carbons alone. If, as they burnt away, a point was reached where the purity and density exceeded the average, the temperature and the light were greatly increased, and a corresponding decrease in purity or density would greatly diminish the temperature and light. The light given by a pair of carbons in an arc lamp would vary 60 to 100 per cent. from this cause alone. This change in the light-giving efficiency during the burning away of a single pair of carbons, and consequent wide fluctuations in the photometric readings, had been the cause of endless trouble to observers. The generator of the current, the lamp, the photometer, the difference of color between the arc light and the standard light, and lastly the observer himself, had all been objected to. It was uncertain what the author meant by "axial" and "bi-axial" measurements. Probably, however, he meant what was ordinarly termed horizontal and angular measurements. A strong protest ought to be raised against the absurdity of taking horizontal photometric measurements of continuous-current arc lights. There was no reason why experimenters should continue making and publishing them without the corresponding angular measurements, unless it was that the latter were a trifle more difficult to obtain; but even that could be easily avoided by inclining the lamp when taking the photomeric readings. At any rate, the commercial efficiency of the light was always taken at the angular measurement, for the simple reason that as all large centers of light, such as electric arc lamps, must be placed high up, in order to avoid floor shadows, the rays below the horizontal plane were of the greatest commercial value. This angular measurement was at least 80 per cent. in excess of the horizontal one, and it was eminently unfair to compare the electric arc, measured thus horizontally, or at its point of lowest commercial efficiency, with the incandescent electric, or any other source of light, the efficiency of which was nearly equal in all directions. The introduction of heat-units into calculations of the candle power efficiency of the lamps seemed to be unwise, and likely to lead to confusion. Surely the expression "candle power per HP." was sufficient to compare the lighting power with the energy. Talking of "Gramme degrees per candle power" seemed like saying "minutes per ounce." In the table where the arc lamps were compared with incandescent ones, the arc lamps were deprived of the 80 per cent. due to the angular measurement not being taken, whereas the average candle power of the incandescent lamps was put at 271 candles per HP., instead of 180 candles, which was certainly the maximum efficiency obtained from such lamps up to the present time, under actual conditions of safe working. With these corrections the efficiency of the arc lamps, compared with that of the incandescent ones, became as 18 to 1. Wide as this gap was, it could not be hoped materially to lessen it, considering that the temperature of the arc carbons was that of disintegration and destruction, whereas that of the incandescent lamps must not be sufficient to soften, or even change, the form of the delicate carbon filaments.

THE BIRMINGHAM AND EDMONTON SEWAGE WORKS.

By THOMAS COLE.

A Paper read before the Civil and Mechanical Engineers Society.

From "Iron."

HAVING visited the sewage works of Birmingham last year, and collected some information thereon, I venture to lay the same before this society, believing that it may prove of interest to many who may be unacquainted with the place and circumstances, and further give rise to a discussion at once valuable and instructive. The population of Birmingham in 1861 was 296,076; in 1871, 342,505, and in 1881, 402,296. The suburban districts of Birmingham, viz., Handsworth, Aston, Saltley, Balsall Heath, Harbone, and Smethwick together give an additional population of 150,000. The lowest point of the borough is at Saltley, where the sewage farm is situated, and this is at 290 feet above mean sea level. The highest point is on the Hagley road, which is 610 above the same datum. At Birmingham one has the advantage of seeing two systems of dealing with the sewage in operation:

First. Precipitation by the lime process;

Second. The intercepting, or dry system;

and I do not think that there is any other town where one would find the details of the two systems carried out to such perfection or where so large an amount of money has been spent or so much energy expended.

To better understand the present position, it is necessary to glance at the history of the difficulties that the authorities have had to overcome in the disposal and treatment of the sewage, and it may be said that in scarcely any other instance has a local authority bestowed more pains to ascertain what was the right system to adopt than the authorities of Birmingham. In the first instance, the sewage was discharged direct into the River Tame, a small stream which at a few miles from the works flowed through the estate of Sir Charles B. Adderley. In 1855 we find the borough surveyor presented a report recommending irrigation. Sir Charles Adderley complained of the nuisance caused in the river by the sewage, and in 1858 on his application an injunction was obtained to restrain the corporation from discharging sewage into the Tame; but the Court, in granting it, accorded time in which the corporation were to construct works to abate the nuisance. In 1859 two subsidiary tanks were constructed near the main sewers, and purification by sand filtration and by upward and downward filtration were severally tried and abandoned. In 1861 the corporation purchased, at a cost of £8000, 28¼ acres of land, in order to obtain access to canal and railway, and for affording additional facilities for dealing with the mud arrested in the tanks. In 1866 Sir Charles Adderley again complained of the state of the river, and the corporation in 1867 took on lease 118 acres of land in addition at a yearly rent of £855, with the object of cleansing a portion of the sewage by irrigation. They caused this farm to be laid out, leveled, and drained, and the necessary roads and bridges, to be constructed, at a cost of £11,250, or at the rate of £750 per acre; but an order of sequestration was obtained in 1870, and another injunction was obtained by Sir C. Adderley, and by owners of property for the purpose of preventing the accumulation of sludge near the subsidence tanks; further acquisition of land was then attempted and failed. In 1871 the Town Council being alive to the defects of the system then adopted, and having an additional stimulus to action by the injunctions obtained against them, appointed a committee to report on the best means of dealing with the sewage of the town. This committee presented a valuable and exhaustive report, and recommended the taking of 2500 acres of land near Kingsbury, about eight miles below the present outlet, and amongst other observations and conclusions passed severe strictures on the lime process. The recommendations of this committee were considered too costly and the whole question was again referred to a special committee, and on

their advice the council promoted a Bill in session 1872 to acquire powers to extend their main sewer to Kingsbury and there to obtain 800 acres of land. This Bill was thrown out on the third reading, and it cost £10,600, leaving the council still in a dilemma. However, to satisfy the requirements of the Court of Chancery the corporation purchased twenty-four acres of land at Saltley for £8000, and further added to that farm by adding to it a purchase of 101 acres at a cost of 29,400. Notwithstanding the committee's report above referred to, the lime process was adopted by Mr. Hawkesley, who, with Mr. Hope, V.C., prepared a scheme for the requirements of the town, and their recommendation being adopted, four additional sets of subsidiary tanks were constructed, to which another large tank has recently been added. In 1877 the order of sequestration was discharged. At this date, notwithstanding the expense incurred by the corporation in clarifying their sewage prior to its discharge into the River Tame, the sewage of adjacent townships with large and rapidly increasing populations was being poured daily into the Tame or into its tributaries without any attempt at clarification. It was therefore resolved to combine under the powers of the Public Health Act, 1875, and the Birmingham Tame and Rea United District Drainage Board was formed and confirmed by Parliament in the following session. The total population of this district is estimated at about 550,000. To meet the additional strain thus thrown on the works the board in 1880 entered into negotiations for the purchase of 867 acres of land at Castle Bromwich, to be used for irrigation from the effluent from the tanks, and in April, last year, the Local Government Board after an inquiry, granted powers to borrow £188,000 for additional land and works.

The Saltley farm, the position of which is shown in red on the plan, has now an area of 272 acres, the subsoil of which is generally of a gravelly nature, with occasional patches of clay. There are three large tanks and sixteen smaller ones, having an aggregate capacity of about 7¼ million gallons. The amount of sludge deposited in the tanks in 1880 was 178,400 cubic yards, or about 490 cubic yards per day, and required an area of 53½ acres of land for digging in the same, or rather more than an acre a week. The average dry weather flow of sewage is about thirteen million gallons per day, the population actually contributing this amount being estimated at about thirty gallons per head. The lime is slacked and ground with water, and mixed with sewage on its arrival at the works, and rather over thirteen tons of lime are used a day.

The sewage next passes through the nineteen depositing tanks with a velocity of about 30 feet per minute through the larger tanks and a little less through the smaller ones. In these tanks the sewage residuum varies in amount and density in proportion to the distance of the tanks from the sewer outfall. The clarified effluent is then allowed to pass by various outlet sluices into the rivers Rea and Tame, or is disposed of by irrigation on the corporation land. The following is the analysis of the effluent taken from the Local Government Report on the Sewage Disposal 1876, p. 36:

Chemical Laboratory, Corporation Sewage Works, Birmingham. Certificate. Sample of effluent water from new precipitating tanks at above, March, 1875. Examined for general impurities. Copy, Jan., 1876.

	Grains per imperial gallon.
Total solid residue containing	58.10
Mineral matter	57.10
Volatile matter	7.00
Suspended matter	1.68
Soluble matter	49.42
Silica matter	0.84
Alumina oxide of iron and phosphates	0.14
Lime	12.22
Sulphuric acid	17.38
Chlorine	9.52
Free ammonia	1.218
Albuminoid ammonia	0.042
Disintegrated animal refuse	0.420
Appearance	clear
Smell	Slightly ammoniacal
Action on test paper	Alkaline

Judging from the appearance of the effluent at the time of my visit, I have no hesitation in saying it was of a character which should not be allowed to go into any river. The sludge is lifted from the tanks by an elevator, and, by means of an elevated trough-carrier, run into beds about 8 yards square, to a depth of about 18 inches, and allowed to drain for a week or two. It is then dug into the

earth and covered with soil. 'Plowing was, for some time, tried, but digging was found to be the only efficient means of amalgamating with the soil. The land is thoroughly drained, and this greatly facilitates the dealing with the sludge. These drains bring the effluent back to the subsidence tanks. The sludged land is very favorable to the growth of the cabbage and mangold; as much as 60 tons per acre is obtained of the latter. The valley of the Saltley Farm is, however, an excessively cold one, consequently market gardening is not as successful as it otherwise might be, as the crops are late. Of all crops that thrive best on sewage, Italian rye-grass yields the best results, but the demand for this has not been large. No nuisance arises from the present method of dealing with the sludge. The borough surveyor states that there are no complaints received from the three thousand houses that are within half a mile of the farm. The cost of dealing with the sludge (lime, labor, &c., but exclusive of sinking fund on capital) was £12,356 per annum, or 1s. 4½d. per cube yard of sludge. Owing to the sharpness of the gradients, and the large proportion of macadamized roads, much of the detritus is carried to the tanks. A small proportion of the sludge was some time ago experimentally converted into cement by General Scott's process, but it was not done to any great extent, and I saw nothing of it at my visit. From the statement of income and expenditure for 1875 and 1876, it does not seem to have been successful, the expenses for the first year of the process being £332, and the income £179, while from the second year the expenses were £300 and the income £150. It is said by some that the lime process as used at Birmingham is merely a temporary means pending the adoption of some more substantial and efficient mode, but the permanent and expensive character of the works tend to preclude such a possibility. The new farm is not yet laid out, but it is intended to connect it with the Saltley farm by a conduit about 2¾ miles long and 8 feet internal diameter. The land is of a very favorable nature and contour, the subsoil being nearly all sand and gravel, and of such a level that 800 acres or nearly the whole may be brought under irrigation by gravitation. It is proposed to lay it out for broad irrigation, except about 40 acres, intended as an intermittent filter bed for use in cases of emergency. About 648 acres will be freehold, and the remainder leased for long periods. It is favorably situated for disposal of produce, being within an easy distance of Birmingham, by which it is well connected by road, canal, and rail. Owing to the acids contained in the sewage from the various galvanizing and other works the liming will still be continued after the new farm is in work, but probably to a less extent, and a considerable amount of sludge now intercepted in some of the tanks will be carried on to the land with the effluent.

General Remarks on the Lime Process.—The Rivers Pollution Commissioners in their first report at p. 52 say, in referring to the lime process at Leicester, Tottenham, and Blackburn, "In all these places the plan has been a conspicuous failure, whether as regards the manufacture of a chemical manure or the purification of the offensve liquid. And further, "the method obviously failed in the purification of the sewage to such an extent as to render it admissible into a river." It is supposed by some that the effect of this and other chemical processes is not only to purify the sewage but to give to the effluent water a manuring principle non-polluting in itself. This, however, is not the case, with the lime process at least, for the fertilizing power of the effluent is not due to any innocuous manurial principle which is added, but rather to the presence of the nitrogenous organic matter which it has failed to abstract. There is this, however, to be said of the lime process that it is the simplest and least costly of any; and it may, perhaps, be said also that the sewage of Birmingham, containing as it does such an abundance of acid metallic salts, is peculiarly suitable to be treated by this process. On the whole, the Saltley works reflect considerable credit on the borough engineer, by whom they have been designed and carried out; kept in excellent order and complete in themselves, they are an evidence of the public spirit shown by the corporation of Birmingham, and will amply repay a visit to any who take an interest in this branch of sanitary en-

gineering, as they offer as good an example of the kind as probably any in the country.

The Intercepting or Dry System.—We now come to the description and the consideration of the "dry process," as carried out in Birmingham, at wharves situated at Rotten Park Street, Shadwell Street, and Montagu Street; the latter, which I visited during last summer, is by far the most important of the three depots. The works at Shadwell Street have, I believe, been partially, if not entirely discontinued, on account of the proximity to the General Hospital. The pail system was established here in 1872, with a view of combating the difficulties met with from the Chancery proceedings above described, arising from the treatment of the sewage from Saltley works. It was, accordingly, thought desirable to adopt the intercepting or pail system, in addition to the lime process then in operation. The pails with their contents together with the miscellaneous contents of ash-pits, are collected weekly, and about 1100 tons of pail contents, are disposed of weekly at the three depots, 466 tons of which representing the contents of about 1700 pails fogether with 506 tons of ashes collected from the premises where these pails are in use, are disposed of weekly at Montagu Street wharf. The superintendent of the department states the number of pans in use in the borough on the 31st December, 1880, was 31,935, and that the carrying out of the work involved the collection, during the year, of 1,621,360 pans and 69,256 loads of ashes. At the Montagu Street works there is an engine house, and two 25 horse-power engines; stack, 260 feet high; three multitubular and two Galloway boilers, the latter being 27 feet 6 inches long and 7 feet 6 inches high, averaging 60 horse-power each, and three of Firman's dryers by Messrs. Alliott & Co., of Manchester, and two by Messrs. Forrest, of Manchester. The collection takes place at night, between the hours of 10 P.M. and 10 A.M., by means of vans or wagons of a somewhat peculiar construction. They are about 13 feet long, and are divided into two compartments, the foremost taking the pails, and fitted with doors closing hermetically, so preventing the slightest escape of smell, and having a capacity sufficient to carry 18 pails, while the rear portion contains the dry ash-pit refuse. This portion of the van is open and hopper shaped. The van, when loaded, weighs about 3½ tons, and is drawn by one horse, special provision having to be made to assist the traction over certain hilly portions of the town. The vans are so made that they can be easily washed with water from side to side. This is done every day, as soon as their work is finished. They are then left with both sides open for the air to play through them and do its part towards keeping them inodorous. The pails are of galvanised iron, cost 10s. each, and are furnished with a well-fitting lid, formed by an elastic washer under the lid, which is kept tight on to the top edge of the pan by the spring on the lid. The spring has a hook at each end, which catches on the hoop round the top of the pan pressed by the spring. The lid makes, with its india-rubber washer, a water-tight joint, and thus hermetically closes it, and so preventing any escape of offensive smell and consequent nuisance during collection. These pails, if brimful, would hold about 14 gallons, but on an average they take about 10 gallons. They are most carefully cleansed, and perfectly disinfected previous to their being sent out.

Most of the poor of Birmingham live in courts, the privies are grouped together, and generally placed in the least conspicuous position. On its arrival at the works the van stops at the foot of a gradient of 1 in 15, when a chain is brought down the incline and attached to the shafts, and thus, with the help of steam, the horse with its load walks up the hill without the least exertion. Arriving at the summit the van is now inside the building, the chain is unhooked from the shafts and the horse takes the van down a passage, stopping at a large cast-iron tank, into which the pails are emptied by hand; the horse then moves a little further and stops the van over a trap door in the floor, and through this door the contents of the rear compartment of the van, the ash-pit refuse, now falls to the floor below, which is the level of the wharf side. Here proceeds the busy operation of sorting. All description of material, such as stones, bricks,

brickbats, and such like, are put into barges and go away to tips. Old iron of every shape and description is sold to a contractor. Rags are picked out and sold, and paper too. Meat and other tins at one time presented a considerable difficulty, but they now find a purchaser who deprives them of the tin and then sells the remaining iron. When the larger materials are taken out, the refuse is thrown into revolving screens; these yield sifted stuff which, being mixed with a portion of the filthy contents of the tanks I have mentioned, is carted off and sold as manure. The cinders and everything combustible goes to the furnaces under the boilers which generate the steam necessary for the manipulation of the works. The sewage is now put through the *Driers*, sulphuric acid being first added to it in the proportion of 30 lbs. per ton, for the purpose of fixing the ammonia. These driers consist of a steam-jacketed cylinder, into the interior of which the pail contents are thrown, and the sewage is kept in motion by revolving hollow arms, through which steam is driven. The shell spindle and arms thus radiating at a high temperature, in combination with the mechanical action, accomplishes the end in view. The vapor is then drawn off by means of an exhauster, is afterwards condensed in a Liebig's condenser, and the liquor is passed into a drain, which discharges it into the adjoining river. To avoid this an experiment is being made to pass the offensive liquid through a filtering medium, which, if successful, will be permanently carried out. These drying machines reduce one ton of sewage to two cwt. three qrs. two lbs., showing that about 11-12ths of the pail contents is only water. The operation of drying one ton is performed in 14½ hours, and the residue, called "poudrette," is extracted from the bottom of the cylinder by means of a door made to open for the purpose. It is then put into sacks and sold to artificial manure merchants at £8 per ton.

Mr. Councillor Martineau, to whose courtesy I am greatly indebted for much information concerning the dry or intercepting system, in speaking of Forrest's driers, says: "We continue to be thoroughly satisfied with the two we have of his make. Our expenditure this year is so much below our estimate that we are buying a new machine out of part of the surplus. It is of a different form from Forrest's; I will not say anything about it until we have tried it. If it is as successful as we hope, we shall, early in the year, ask the council for a very large sum of money to enable us to make poudrette of *all* the pail stuff taken to Montagu Street. The total cost of removal of night soil and the collection and disposal of the house refuse in 1880 amounted to £42,996, and the total receipts from the sale of the different products amounted to £7,694 11s. 8d., which leaves a sum of £35,297 5s. 9d., as the net cost to the borough of Birmingham.

It is contended in defence of this system that it tends to isolate contagious diseases, inasmuch as the fœcal matter is kept from spreading its poisonous germs, as would otherwise be the case in the common sewer, and as a practical proof of the sanitary improvement of the town, it is pointed out that the death rate at the date of the introduction of the pail system in 1872 was 24.02 per 1,000 inhabitants, and 5.2 of these deaths were due to zymotic diseases, whereas the death rate now stands at 21.49 per 1,000, and of these only 3.2 were due to zymotic diseases. There is no doubt that in India and other places where the water supply is often not plentiful, and the question of sewage disposal presents great difficulties, the systematized pail system would afford great advantages. It is further urged that the use of pails is eminently suitable to those tenements where the water-closet system is carelessly or wilfully abused, and the apparatus is constantly getting out of order; and owners of these properties hail with favor the adoption of the system as an immense saving to them. Unquestionably this kind of property presents a good deal of trouble to effectually deal with their sanitary arrangements, and I would draw your attention to a form of closet to utilize the waste water of a house for flushing purposes, called "Fowler's closet," which has been found admirably suitable to places where no other has been found to answer. The system appears simple, and has been adopted in several towns with the most satisfactory results, the surveyors to the different localities speaking of it very highly. The

surveyor to the Local Board of Felling says: "I consider it a boon to the public at large, more especially to the working classes, it being a simple and efficient arrangement, and further, there is no machinery, consequently it is most suitable for tenement and working-class property." Being simple in construction, it is quite impossible for the system to get out of order. There is no expense in obtaining towns water, as all the slops and refuse water from the house and yard pass through the closet. There is not the slightest smell or nuisance where these closets are adopted.

Returning to the dry system, the author has only further to state that at the annual congress of municipal engineers held in Birmingham last year the members of that association very warmly condemned the system as dirty and demoralizing. The royal commission appointed to inquire into the drainage of Dublin says of the pail system: "That the collection of the city excreta by means of movable pans, or by the process of so-called dry conservancy, will cause more nuisance and be more costly than water carriage. The nuisance will be greater, because there will be a retention of the excreta on the premises, and the cost will be greater by the amount of labor necessary to collect the excreta, and also because there is no practical mode of converting the excreta into a portable manure which will pay the incidental charges." Mr. Rawlinson says his views respecting Dublin equally apply to every other town in the country. One of the inpectors of the Local Government Board has said, however, that the works at Montagu Street, which I have described to you, and which he visited, were, and would be, a success. The system is admirably carried out, and, as far as circumstances will allow, wonderfully free from offence.

The Edmonton Sewage Works.—Some of the members of this society visited these works last summer, and our numbers being few on that occasion, there must have been many who were unable to avail themselves of the excursion, and it is with the idea that some description of what we saw may prove of interest to the absentees on that occasion that I venture to put before you the following notes, not out of place, perhaps, as an appendix to this paper:—The Edmonton Sewage Works are situated about a mile from the town, and close to the main line of the Great Eastern Railway. The population in 1877 of the district was 15,000, and that provided for at the works 60,000. The area of the district is 7854 acres, or about 12 square miles. The area of the sewage farms is 114 acres, of which 8 acres are used as a downward filter planted with osiers. Twenty-one acres are used for irrigation purposes, and the remainder is let to farmers. The sewage is treated on its arrival at the works on a modification of the lime process known as Hille's system, in which lime is the chief precipitant, the patent consisting in the addition of magnesium chloride and tar. The sewage, varying from 80,000 to 100,000 gallons per day, is brought to the outfall works by a 3-feet 6-inch sewer, first passing through a screen in a penstock chamber, which separates the larger materials; it then enters a second chamber, from which it may at pleasure be let out direct on to the land without entering the depositing tanks. A 10 horse-power steam engine here works a Gwynne's centrifugal pump, and regulates the necessary amount of disinfecting material which is added. The sewage then flows into a collecting reservoir which is underground, built in brick and roofed in, and capable of holding two million gallons; from here it is lifted into the deposit tanks when precipitation is carried out. The pumping, mixing, and supply of chemicals is performed by two 14-inch pumps worked by two 10 horse-power engines contained in an engine-house, which include the machinery and the mixing apparatus. The sewage is lifted and delivered from the reservoir into an iron cylinder 5 feet high by 5 feet in diameter. In this cylinder sewage and disinfecting compound meet. Another cylinder of like dimensions, fitted above that which receives the sewage, contains an agitator which is driven from the engines and holds the disinfecting com pound; this has to be dissolved in, and diluted with, sewage, as no pure water is available. The three deposit tanks are built in concrete above ground; they are side by side, and divided by two concrete walls. Each tank is 200 feet long, 30 feet wide, and 7 feet deep, with the

bottom of each sloping towards its centre with an outlet pipe, through which the sludge is emptied by a subterranean channel to the sludge beds. The sludge bed is about 150 feet long, 30 feet wide, and has an average depth of 2 feet 6 inches. The sludge may be deposited here when not wanted, or it may be delivered at the penstock chamber before alluded to, and to which it is conveyed by an open channel. At the date of the visit there seemed to be some difficulty in getting rid of the sludge as the beds were full, and bore the appearance of having been full for some time, but I am informed that there are now three sludge beds in use at these works, and these are used alternately. When one of the beds is filled the moisture is drained off, and the sludge is removed and used by the farmers as manure. They fetch it in carts, and pay some 2s. to 3s. per load. There is no accumulation of sludge at all now, as the stuff is produced and removed from the tanks, the sludge beds are filled and used in rotation. The demand for the sewage manure is considerably increasing since the quality and its value have become appreciated. The osier beds occupy an area of about eight acres, they are underdrained some three feet deep, and are said to take some two or three days' sewage running. They were not in use at the time of the visit, the various sewage channels being dry, caked hard, and generally neglected. The growth of these osiers is, nevertheless, stated to be a success, and the first year's growth is reported to have yielded a profit. To return to the tanks. These are so arranged that they may be used singly or all three at the same time, the water passing from the first into the second, and when these are full from the second into the third tank. From the overflow of the third tank the effluent water passes either direct into the river Lea, after running along some mile and a half through ditches, or it may be first passed on to the filtering beds or on to the field, 21 acres in extent, used for irrigation, and from here find its way to the river. There are 10 acres laid out for water-cress cultivation, which the Board of Health let at £10 per acre per annum. Only the purified effluent is used for these beds. The quality of the cress is said to be excellent, and the man who rents the 10 acres is doing extremely well. Mr. Hille, to whose courtesy I am indebted for much of the information contained in these notes, reports that the return from the sale of the sludge and produce of the farm cover to a considerable extent the cost of the treatment and disposal of the sewage at these works.

THE EFFICIENCY OF SECONDARY BATTERIES.

By E. REYNIER.

Translated from "Comptes rendus de l'Academie des Sciences," for Abstracts of Institution of Civil Engineers.

Work by secondary batteries includes two phases—the charging of the accumulator by the action of an external electric source, and its discharge in the circuit worked. Each of these operations includes a loss. In seeking the expression for efficiency, let E_0 be the initial electromotive force of the source, R_0 its resistance, E the electromotive force of the secondary battery, R its resistance, E_1 the difference of potential at the two extremities of the conductor worked, R_1 the resistance of this conductor, t the time of charge, t_1 the time of discharge. The work T_0 expended in charging will be (supposing it to be constant) $T_0 = E_0 \frac{E_0 - E}{R_0 + R} t$. The work T utilized in the resistance worked will be $T = \frac{E_1^2}{R + R_1} t_1$. To find the ratio of these works, it is necessary to express t_1 in function of t. It may be arrived at by considering that the quantity of electricity Q is the same in the circuits of charge and discharge (which needs experimental verification), and that this quantity is proportional to the products of the quanti-

ties of the currents by the times, whence the equation

$$\frac{E_0-E}{R_0+R}t=\Phi=\frac{E_1}{R+R_1}t_1;$$

and whence $$t_1=\frac{t\frac{E_0-E}{R_0+R}}{\frac{E_1}{R+R_1}}.$$

By substitution, the efficiency

$$\Phi=\frac{T}{T_0}=\frac{E_1}{E_0}.$$

The efficiency is thus expressed by the ratio between the difference of potential at the two ends of the resistance worked and the initial electromotive force of the source of electricity; it is independent of resistances and of the values of the times of charge and discharge. This is based on the supposition that the work produced was the heating of a resistance; if the discharging current actuated a circuit which was the seat of an electromotive force, in an electric motor for example, the expression for efficiency would not be altered. But E_1 should then express the contrary electromotive force of the motor at the origin of the induction.

In practice, the resistances of the circuits should be taken into consideration. On account of the low internal resistance of M. Faure's secondary battery, 80 per cent. efficiency can be attained with advantageous conditions of charge and discharge. The constants of the Faure battery are, for the small size of the 7.5 kilogrammes battery, E=2.15 volts, R=0.006 ohm. making $E_0 = E \times 1.1 = 2.36$ volts, $E_1 = E\,0.9 = 1.93$ volts, $R_0 = R = 0.006$ ohm, $R_1 = R \times 9 = 0{,}054$ ohm. The work expended during charging will be $\frac{E_0^2-EE_0}{g(R_0+R)}=4.24$ kilogrammeters per second and per couple, which admits of saturating the battery in a charging time much shorter than is usual. The work returned per second and per couple during the discharge will be equal to $\frac{E_1^2}{g(R+R_1)}=6.3$ kilogrammeters. As to efficiency, it is, under these conditions, $\frac{E_1}{E_0}=\frac{0.9}{1.1}$, or 81 per cent.

PLATE-WEB GIRDERS.*

From "The Building News."

ALTHOUGH the tendency of modern engineers is apparently to adopt very large braced girders for bridges wherever possible—the advisability or necessity of such immense structures not being always considered, but rather the hope of obtaining reputation on the theory that genius varies directly as the span—by far the greater number of girders which are erected in this and other countries are of the plate-web type.

Several very interesting and elaborate papers have lately either been read at the institution or published in the journals, on the subject of large braced girders, and the subject has been so thoroughly treated, both as regards the weight of the structures and the strains due to every possible condition of loading and wind pressure, that little more need be said on the subject; but the author would wish, in passing, to call attention to the elaborate and unwieldy formulæ which are given to solve the different questions, and would ask if equally reliable results could not be obtained by using simpler and less complicated formulæ, which would reduce considerably the liability to error in the calculations. As an illustration, the following formula for obtaining the weight of girders with parallel flanges is taken from the paper on "Girder Bridges," by Mr. Max Am Ende—

$$Q=\frac{1}{\left(\frac{s}{0.00213}-\frac{nl}{2}\right)D-\frac{D^4}{2}-\frac{1}{6}}\left\{(P+M)\frac{L^2}{6}+(P+M)D^2+(B+1/6\,M)\frac{1D}{2}n+W\right.$$

$$\left.(0.2D+18)\left(\frac{0.15}{(.3f-s)}\frac{B}{L}\left(\frac{9}{48}\frac{L}{D}+\frac{1}{2}\frac{D}{L}+\frac{1}{8}\right)\right.\right.$$

$$\left.\left.+\frac{1}{2}\frac{L}{D}+5/4\right)D^2+\frac{s.0.0073}{0.00213}D\sqrt{(D-10)(B-6}\right\}$$

* Read before the Liverpool Engineering Society, March 29th, 1882, by John J. Webster, Assoc. M. Inst. C.E.

As this formula is simply given to show its great length, it is not necessary to explain the different symbols beyond stating that Q is the weight of the girder with parallel flanges, with bracing bars placed at an angle of 45°. Now, suppose that any one using this formula, after filling sheets upon sheets of foolscap, were lucky enough to wade through its entire length without making an error in his calculations, would the results obtained be of such marvelous accuracy as to repay him for his trouble? The author thinks not, and for the following reason: Suppose a long chain had to be made to stand a certain load—say, 100 tons; now, if the links were to be made of some material which was well known, such as wrought iron or steel, it would be an easy matter to calculate very closely what the size of the links should be; and the formula for such a calculation would be accurate and could be depended upon. But suppose, now, that instead of all the links being of this known material, some of them were of a material about which there was nothing definite known as to its breaking strain or other qualities, what would be the value of the formula then? It would be simply valueless. It might possibly give correct results, but it could not be relied upon in any way; and until more is known of the nature of these mysterious links, an elaborate formula is simply useless, and would only give results which may be termed "falsely accurate." Now, the formula quoted is very much like the chain, and is full of mysterious links, which at once vitiate what would apparently be accurate results. In the first place, the pressure of the wind is a factor in the investigation, and what more mysterious link is possible? What is known about the pressure of the wind, even as to actual pressure, or as to its local action on large exposed surfaces? It is only necessary to examine the statements made by different authorities to at once find out how little really is known, that the different authorities do not agree, and in fact, to find nothing but hopeless confusion. Another mysterious element is the factor of safety; for suppose it is known exactly to what amount each member of a structure is strained with certain loads, what is to determine the strain per square inch which the material should bear with a maximum load?

This is simply a matter of opinion, and cannot be fixed definitely either one way or another; but taking the practice of different engineers, a variation of opinion is found to the extent of at least 25 per cent., which would, of course, materially affect the weight of girders. Again, this factor of safety would have to vary in the same structure, for in some cases—as, for instance, the lattice bars at the center of a braced girder, or the abutment ends of the top and bottom flanges of parallel straight girders—the amount of material really required is so small that it could not be adopted practically, and the section is increased accordingly; so it often happens that the amount of material required under certain circumstances is determined not by abstruse calculations, but by the judgment of the designer.

Taking all these things into consideration, it seems very evident that a formula containing all these uncertain elements cannot give anything but approximate results, and that being the case, equally reliable and accurate results can be obtained by using formulæ which are more concise and which thus reduce the liability to error in the calculations. It must not be thought, however, that the author is advocating in any way a rule-of-thumb method of designing girders—far from it; and he would mention as a type of what he considers good and reliable formulæ, tables and diagrams—those compiled by Mr. B. Baker—where every detail as to the strains and weights of girders can be determined sufficiently accurate for practical purposes for most types of girders, from the smallest to the limiting spans.

The plate-web girder is considered by many to be the simplest form of girder, the calculations required for determining the strains and subsequent distribution of the metal being also supposed of the simplest kind, and requiring very little consideration. Thus we find that girders of this class are often designed and constructed in a very reckless manner, very little consideration being given to the arrangement of plates, designs of joints, and other so-called minor details—everything being considered correct and safe so long as there is "plenty of metal."

Instead, however, of the plate girder being of the simplest form, it is in reality one of the most complex, and the consideration of it involves one of the most complicated problems which could possibly occur, and which cannot be so easily determined as the strains in the different members of a braced girder. The calculation of the strains in the flanges does not offer any special difficulty, the strains being easily determined by the well known formulæ; but when the strains in the web have to be calculated, innumerable difficulties at once present themselves. The web, of course, has to be constructed to withstand the vertical strains which are transmitted from flange to flange, and which strain is called the shearing strain. But the question is, how are these strains transmitted, and in what direction? This point has been thoroughly investigated by two of the first mathematicians of the age—viz., Professor Airey and by Mons. Bresse, the results of the investigation of the former gentleman being communicated to the members of the Royal Society in 1862.

There was, certainly, before this time a correct general notion of the nature of the strains in the web, but no actual theory had been advanced by means of which the strains could be mathematically expressed. From the experiments made by Mr. Stephenson on the model tube for Britannia Bridge and the mathematical investigations of Professor Airey, it was found that diagonal strains, both compressive and tensile, occurred in the web, and that the angle of the diagonals was about 45°. It was the consideration of this that made Mr. Stephenson advocate so strongly the adoption of web plates in preference to lattices, and he argued that it was only necessary to conceive a lattice girder, with the lattice bars close to one another, to have at once a web-plate girder with two webs, one web acting in compression and the other in tension; and as there is nothing to prove that a bar in tension in direction of its length may not at the same time resist a compressive strain in direction of its width, it follows that only one-half the section of the web would be necessary if the metal were in one piece instead of being divided. This view was also supported by Professor Airey, who commenced his investigations by proving the theorem that "whatever be the number and direction of the forces of compression and tension, their combinations may in all cases be represented by the the combinations of two forces at right angles, these forces being sometimes both of compression and sometimes both of tension, and generally unequal in magnitude." He then investigated the condition of two such forces acting at each point of the web, paying particular attention to the condition at the ends of the girder resting on the pier. In all vertical sections of the web he found both a tensile and compressive force resisted by similar forces of equal amount acting in reverse directions; but at the ends of the girders these opposing forces did not exist, the vertical pressure which a horizontal portion of the web had to resist at the base being equal to one-half the distributed load and reduced uniformly to the top of the girder.

Having shown the nature of the stresses in the web, it remains to be shown how the strength of a web plate is to be calculated in designing a girder; and here difficulties and wide differences of opinion at once present themselves. It is astonishing how little this question appears to have been taken into consideration even by persons who are constantly designing girders; and the majority of persons, when asked by what rule they determine the thickness of the web, have not been able to give a satisfactory reply; and most of them have admitted that they never calculate it, but make it what they think is sufficient. This accounts, no doubt, for the number of curious plate girders which may be occasionally seen on their way to the site of some large building in course of erection, or even sometimes to a railway in course of construction.

Taking it for granted that the stresses in the web do act in a diagonal direction, at an angle of 45°, it will be as well to see how different authorities then treat the question of determining the necessary thickness.

Professor Reilly, of Cooper's Hill College treats it as follows: "Let N be a very small cubical element in the web. The diagonal of the square in the line AB is the direction of a normal compressive stress of equal intensity to the

shearing stress, acting in all sections of the small cube which are normal to that diagonal; the other diagonal being the direction of a similar tensile strain. Consider a narrow diagonal strip of the thin web plate, whose mean fiber is the diagonal of the square produced both ways to meet the top and bottom angle irons of the girder, and whose length $=l$. The web may be conceived as made up of a number of such strips, and further, they may be considerd as isolated—a supposition which is much on the side of safety, as each strip will be in the condition of a long diagonal pillar or stout encastre at each end, by being gripped between the angle irons; the least breadth of the pillar being the thickness of the web. Then determine the intensity of the resistance to failure by lateral bending or buckling of such a diagonal pillar, and compare it with the intensity of the shearing stress on a vertical section on which the shearing force is greatest, which is close to the end of the span—

Let po = force required to buckle the pillar.

qo = shearing stress on a vertical section.

Then $\frac{po}{qo}$ must give a sufficient factor of safety,

which may be fixed as low as 2, considering that the diagonal strips which have been treated as isolated strips are really connected with one another, so as to form a continuous web, and by their mutual support oppose a greater resistance to buckling than is given by the calculation for *po;* how much greater there is at present no known method of computing." The following is an example of a cross girder worked out:

Let the distance between the rivets of the angle irons be 21-in., then the length of the pillar taken in the angle of 45° will be

$$21\sqrt{2} = \text{say } 30\text{-in.}$$

Let t = thickness of plate = say $\frac{3}{8}$,
then by the well-known Gordon's formula for columns, deduced from the experiments of Hodgkinson, the resistance of the pillar to lateral flexure is

$$\frac{36000}{l^2} = \frac{36000}{900}$$

$$po = 1 + \frac{36000}{3000t^2} \quad 1 + \frac{36000}{3000 \cdot 64} = \frac{36000}{3.13}$$

$=5.14$ tons per square inch of the section of the plate.

Let the shearing force at a section near the end of span $=22$ tons,

$$\text{the } qo = \frac{\text{shearing force}}{\text{sectional area of web}} = \frac{22}{\frac{3}{8} + 28}$$

$=2.1$ tons per square inch,

then the ratio $\frac{po}{qo} = \frac{5.14}{2.1} = 2.45$, which Professor Reilly considers is more than sufficient for a factor of safety.

Professor Rankine treats the matter in a somewhat similar manner, but has entirely different notions as to the factor of safety to be employed. In his "Manual of Civil Engineering," page 529, he says:

"The thickness of the web is seldom made less than $\frac{3}{8}$-in., and, except in the largest beams, is in general more than sufficient to resist the shearing stress. In those beams in which it becomes necessary to attend specially to the power of the vertical web to resist the shearing action of the load, the amount of that shearing action is to be computed by the formulæ of Art. 161, &c. It is, then, to be considered that the shearing stress at the neutral axis is equivalent to a pull and a thrust of equal intensity, inclined opposite ways at 45°, and that the vertical web tends to give way by buckling under the thrust, so that its ultimate resistance in pounds per square inch is given by the following expression:

$$po = 1 + \frac{36000}{\frac{s^2}{3000t^2}}$$

t being the thickness of the plate and s the distance measured along a line inclined at 45° to the horizon, between two of its vertical stiffening ribs, or if it has no such ribs, between the upper and lower horizontal ribs. The intensity of the shearing action of the working load should not exceed one-sixth of the resistance given by the above formula."

That is to say, taking the same symbols as Professor Reilly,

$\frac{po}{qo}$ must not be less than 6.

Mr. Bindon Stoney, in his book on "Strains in Girders," in speaking of the vertical strains in a web, remarks as follows:

"This vertical strain has been aptly named the shearing strain; but few writers until the last few years have noticed the practical results which follow from the fact that this force can be communicated from section to section only through the medium of some diagonal strain. Respecting the exact directions of the strains which this shearing force develops to a continuous web, we know nothing positively; it is probable that they assume various directions, crossing each other like lattice work—some vertical, some diagonal, and perhaps some curved. However this may be, we know that certain of them must be diagonal, since the weight which is a vertical force produces strains in the flanges which are longitudinal, through the medium of the web, which, in fact, fulfills the part of bracing in a lattice girder." Further on, in speaking of long plates, he says: "An isolated plate under compression may be regarded as a wide rectangular pillar, or as a number of square pillars placed side by side, and it will therefore follow the laws of pillars, so far as deflection at right angles to its plane is concerned. If, however, the plates form the sides of a tube (as in the web of a girder), this rule does not apply, since in that case they yield by buckling or wrinkling of a short length, and not by flexure; being held in the line of thrust by the adjacent sides, which enables them to bear a greater unit strain than if not so supported along their edges." Further on, when speaking of how the thickness of the web is to be determined, he says: "When calculating the area of a plate web from the total shearing strain, it is a safe rule to adopt four tons per sectional area of web as the maximum shearing strain; but this rule gives no idea of the amount of material requisite for stiffening the web, and which can only be determined by experience in each separate case."

Mr. B. Baker contributed a very interesting paper to the Institution in 1880 on the "Practical Strength of Beams," from which a few extracts will be made, as bearing upon the present subject.

After experimenting on a large number of girders, details of which may be found in his paper, he says: "The strength of a plate web, according to Professor Airey, Mons. Bresse, and nearly every other mathematician, is governed by the resistance of the web to the diagonal compression due to the shearing stress. This may be practically true in some cases, but it was not so in that of the 24in. by ½ web of girder *g*, or the shearing strain sustained would have been double the 4½ tons per square inch, which crippled the web; neither was it approximately true in the instance of some girders with 36 by ¼ webs which the author tested, with the view of determining the real nature of the stresses in a plate girder as generally constructed." He then describes the girders, and the result of the experiments, and says: "The maximum shearing strain was 45 tons, or at the rate of 4.3 tons per square inch of the gross section of the web. The resistance of the thin web to diagonal compression would be less than a third of this, so that the strength was obviously not governed by the conditions laid down in ordinary theory. The permanent set of 1-16th of an inch could not be due to excessive compressive strains on the web, because the total deflection of the girder was far too small to permanently bend such a long elastic column as that constituted by the ¼ web. It could only be due, therefore, to the stretching of the web under tensile strains. From a careful consideration of the phenomena exhibited, the author was led to the conclusion that at a point in the center of the web plate experimented upon, when by the ordinary theory the diagonal strains would be about 4½ tons per square inch, both in tension and compression, the strains were, as a matter of fact, 11 or 12 tons in tension, and half a ton or a ton in compression." Mr. Baker verified his experiments on the preceding girder by numerous others on five girders of equal size, but with varying proportions of flange and web, and obtained practically the same results. He also made models of the girders to scale with wooden

flanges and stiffeners, and paper webs, and tested them to destruction, when he found the phenomena observed in the full-sized girders were repeated to exaggeration in the models, the lines of stress being shown with conspicuous clearness.

The latter experiments proved more suggestive than all the experiments on the iron girders, and all the mathematical investigations on the subject; and Mr. Baker says that "after witnessing them there was no difficulty in forming a clear conception of the nature and intensity of the strains occurring in a plate web as ordinarily constructed," and further states that "the local weakness in the preceding girders, which would have determined failure before the full strength of the flanges had been developed, was again thinness of web. In the three cases cited, the strengthening of the locally weak portions would be a subject rather for practical experience than of theoretical investigation."

He then states: "So far as web plates of medium size are concerned, he is of opinion that the general condition laid down by Mr. Chanute, in his specification for the Erie Railway bridges, meets all the requirements indicated by experiment. These are: that the shearing strain shall not exceed half that allowed in tension on the bottom flanges of a riveted girder, and that when the least thickness of web is less than 1-80th of the depth of the girder, the web shall be stiffened at intervals not over twice the depth of the girder." Mr. Baker then concludes by saying: "Hundreds of experiments might be cited to show that the practical strength of a beam, at low strains as well as at high strains, is dependent, to an important extent, upon other considerations than those included in the mathematical investigation. In other words, it is certain that the less strained fibers in a beam 'practically' help their more severely strained neighbors at low strains, as well as at high strains, although 'theoretically," as M. Barre and St. Venant and others have shown, the assistance would appear to take effect at high strains only."

Having briefly stated the opinions of different authorities, it now remains to sum up the various theories which have been advanced, and, if possible, to deduct some practical result. It will have been seen, however, that the opinions expressed are so widely different, that to attempt to reconcile one with another would be utterly impossible; and it is only necessary to work out an example by different methods to at once see the amazing discrepancies in the results. For instance, if the calculations for a bridge, say of 100ft. span, having two outside girders, carrying a double line of rails, be worked out, it will be found that the thickness of the web plate at the ends will vary, according to the different formula adopted, from about $\frac{1}{2}$-in. to $1\frac{1}{4}$-in. thick. The method adopted by Professor Rankine, Professor Reilly and others, it has been stated, is to treat the web plate as so many isolated pillars, fixed at the end. Now, the question is, Is that a legitimate way of treating the question? The author is strongly of opinion that it is not. In the first place, the conditions are certainly not those of a loaded isolated pillar, for, as Mr. Stoney remarks, they certainly receive support from one another, and from the top and bottom angle irons and stiffeners; again they are crossed at right angles by strips of metal in tension, which must also strengthen them, and the length of the pillars gradually diminishes at the top and bottom of the web as they appoach the junction of the vertical stiffeners and the top and bottom angle irons, and, being shorter, are stiffer, and so add lateral strength to each ideal pillar. If the factor of safety $\frac{po}{qo}$ as given by Rankine be adopted, the thickness of web will be out of all proportion, being far too thick; but Professor Reilly takes the above conditions into consideration, and admitting that there is no known method of computing the exact resistance to buckling, gets over the difficulty by adopting a very low factor of safety, thus obtaining reasonable results. But if the formula for columns has to be so cut and carved to make it give satisfactory results, why use the formula at all? Equally satisfactory results could be obtained by using any other formula, say, for instance, the one for obtaining the bursting pressure of a boiler; by making the shearing stress equal to the boiler pressure, and the length of the column equal to the diameter of the boiler, the

thickness of the web could be obtained by working out the usual formula for bursting pressure, and then dividing by some wonderful constant to make it fit.

The fact of having to use such a doubtful factor of safety, and the experiments made by Mr. Baker, prove conclusively that the web cannot rationally be conceived as a number of isolated columns, and therefore to treat it as such appears, on the face of it, most unreasonable and decidedly incorrect. The author's practice has been to allow a shearing stress of 2½ tons per square inch on the gross vertical sectional area of the web for large girders, and 3 tons per square inch for small shallow girders; the spacing of the vertical stiffeners being determined, not by theory, but from the results of practice. This method has been condemned by some engineers as being a rule-of-thumb method; but when it is supported by such an authority as Mr. Baker, who has proved by experiments and by reasoning that the "practical strength" of beams is different from that dictated by theory, the author feels perfectly justified in adopting and advocating a rule which is founded on actual experience, and which gives far more reliable results than those obtained by doubtful theories.

ON THE DETERMINATION OF THE QUALITY OF IRON AND STEEL.

By PROF. LUD. TETMAJER.

Translated from "Eisenbahn," Zurich, for Abstracts of the Institution of Civil Engineers.

In a previous article on the same subject the author gave his reasons for objecting to the method of determining the quality of iron and steel as recommended by the Commission of the German Railway Union; namely, by means of the breaking strains and the contraction, and substituted for it the working capacity, *i. e.*, the product of tensile breaking strain into the elongation.

$$a=\eta\beta\lambda$$

where η is constant for a certain kind of metal. Further experiments by the author have confirmed the constancy of η, and have shown that even for different brands of the same kind its variations are of no practical importance; the different brands at present in the market can therefore be treated together in groups on the basis of the working capacity.

In the above equation a determines the class of quality of a kind or group, η the kind of the material. Consequently, $\frac{\text{minim.}\,a}{\eta}$ is constant for a certain class, and this constant

$$c=\beta\lambda$$

is the coefficient determining a class, β being given in ton per square centimeter, and λ in percentage of a given length of bar. The law of dependence of β from λ is expressed by a hyperbola, whose asymptotes are the axes of the system, and the different classes of quality can be distinguished from each other by pieces of hyperbolas.

Availing himself of the results arrived at by prominent experimentalists, and having regard to the interests of both railway companies and iron masters, the author has worked out the following classification:

A. Puddled iron (four classes).

I.	quality,	$c=68$	ton per cent.	
II.	"	$c=48$	"	"
III.	"	$c=34$	"	"
IV.	"	$c=24$	"	"

B. Cast malleable iron or steel (one class).

$c=93$ ton per cent.

[For example, iron of a breaking strain=3,200 kilograms per square centimeter, and an elongation of 12 per cent., has a $c=38.4$, and would accordingly rank in class III.—Ed.]

The limiting figures for the various classes would have to be agreed upon from time to time, although it is not

likely that those of group A. will be greatly modified. The results of experiments with this material, when plotted on a system of co-ordinates β and λ, are spread very evenly over the range of the above four constants; the results from material of the group B., on the other hand, lie much closer together when plotted on the system, and a hyperbola $c=93$ can be drawn easily; in such a way that the great bulk of the plottings lies above, and not very far above it.

Graphical interpretations of the same experiments on the basis of breaking strength and contraction did not bring to light any rule, while the grouping of the plottings according to β and λ seems to confirm the correctness of the author's method. The curve $c=93$ is, in the opinion of the author, still too low; but it is higher than the lines proposed by the German iron masters, which are so low that, according to them, a consumer would be obliged to accept almost anything that is produced.

The conditions of specifications with reference to quality of metal would have to be stated in the following forms (given as an extract):

Prime rivet and bolt iron.

Min. tensile strength $\beta=3.8$ ton per sq. centimeter.

Coefficient of quality $c=68$ ton per cent.

Round bar iron for machinery and bridges.

Min. tensile strength $\beta=3.6$ ton per sq. centimeter.

Coefficient of quality $c=48$ ton per cent.

Cast-steel rails.

$\beta=$from 5.2 to 6.4.
$c=93$.

Cast-steel tires.

$\beta=$from 4.6 to 5.5.
$c=93$.

Cast malleable iron boiler plates.

$\beta=$from 3.7 to 4.8.
$c=93$.
&c.

CURVES AND CROSSINGS FOR RAILWAYS.

By S. W. ROBINSON, C. E., Prof. Mech. Eng., Ohio State University, Columbus, Ohio; Member of the Board for Inspectors under the Hon. H. SABINE, Commissioner of Railroads for Ohio.

I. FORMULAS AND TABLES FOR EASEMENT CURVES AS ADAPTED TO FIELD PRACTICE.

SINCE the article on Railway Economics* was written the problem of the "easement" curve has been pursued farther with a view to putting results and facts in the most convenient shape possible for use by field engineers.

It might at first be imagined that the complexity of practice with any easement curve must necessarily be so great as to render its use entirely out of the question. But a little consideration of the table of quantities given below will show that this is not the case; indeed, from the fact that the quantities needed are already made out and given in tabular form, it may be found easier to construct easement curves than circular curves. Though a great variety of easement curves is possible, only one is necessary, and when this one is selected, all the quantities pertaining to it which are needed in practice can be at once computed and tabulated, the table being extended to include any case of practice. This is seen to be possible from the fact that any proper easement curve must be a sort of a spiral, beginning with an infinite radius at the point of departure from the straight tangent, and extending to where the radius of curvature becomes equal to that of the principal circular curve to be joined with it. Hence the table should be carried to the smallest admissible radius of principal circular curve; which table representing some one carefully-selected spiral or easement curve, is ready for every case, and furnishes deflection angles already made out for part of every curve to be run in practice. Indeed it is possible by aid of the table to run in a complete railway curve between any two tangents, consisting wholly of two portions of the easement curve in common tangency,

* May Magazine.

and without computing a deflection angle, nor summing them for total deflections.

On the other hand it is well known that some species of easement curve is absolutely necessary for the transfer from a tangent to a circle curve without the disturbance of the lateral equilibrium. Hence easement curves are a necessity to perfect track.

A number of curves have been proposed for effecting this easing, and a few of them have been used in practice. But probably no rules for practice heretofore published came nearer to realizing the needs of practice than those presented in a most excellent article in the *Railroad Gazette* of Dec. 3, '80, by Ellis Holbrook, C.E., of Richmond, Ind. A table is there given which contains most of the quantities required. Mr. Holbrook is introducing these curves on the Pan Handle Railroad.

The methods of that article are found of such rare merit that they are followed largely in this, the chief difference being in additions which aim to more fully anticipate the needs of practice. A different curve is, however, adopted in the present instance for reasons soon to be given.

The curve of Mr. Holbrook is a spiral with infinite radius at the tangent point, and with the radius of curvature varying inversely as the distance from the tangent point as measured along the track.

From the general considerations offered in the principal article above, under "The Track Line," it appears that the spiral there adopted is one in which the radius of curvature varies inversely as the *square* of the distance from the point of tangency. The object in choosing the square was to reduce disturbances, due to entering upon the curve, to the least possible value, as fully discussed in the principal article. For the same reason the law of the square is still retained.

The elevation of outer rail on curves is well known to be inversely as the radius of curvature of the track curve. Hence in the present case the elevation varies directly as the square of the distance from the point of tangency. By choosing the law of the square, the acceleration of the car in its rotation on a longitudinal axis as already explained is made constant, and to a person sitting at the extreme side of a car, the only sensation due to entering upon a curve would be that of a slight increase of weight, or of decrease, as the case might be; and which would continue constant throughout the easement curve. But where the variation of elevation and of consequent rotation of car on a longitudinal axis is as the first power of the distance from the tangent point of the curve, the elevation of a person at the extreme outside of the car would be uniform as the car rotates, but that uniform rate would have a sudden beginning at the initial point of the curve: the action being like that of imparting a uniform motion upward to a body from a state of rest by an instantaneous knock. Though the practical effect of this instantaneous impulse may be declared insignificant; yet from a scientific standpoint it is incorrect, and the law of constant acceleration is more acceptable.

RULES FOR RUNNING THE EASEMENT CURVE.

Let Fig. 1 represent a simple case where two tangents intersect at C. Take D and H as tangent points, from which a circle curve shown by dotted lines might be put in from a center O.

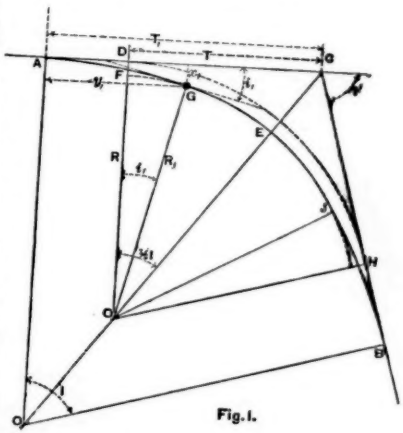


Fig. I.

Let A and B be the tangent points for the new curve in which AG and BJ are the equal easement curves, and GJ the principal, or intermediate circle curve. Perpendiculars at A and B meet in O, at an angle equal the angle of intersection of the tangents. The circle may be ex-

tended back from G to F where its tangent is parallel to AC. O is taken a common center to the dotted circle DH, and the principal circle GJ.

In running the curve in the field, we may start at the point A. With chords and tabulated deflection angles, run to G; then set the instrument at G and run the circle GJ; then go to B and run the easement curve BJ. To eliminate inaccuracies it may be advisable to run the two easement curves first. Then with the instrument at G examine the total deflection angle for J. If the discrepancy is small, set on J to dispose of it, and connect G and J.

To conveniently express relations between quantities, take

I = the intersection angle at C_1 = DOH = AO_1B. Then DOC = $\frac{1}{2}$I.

R = the radius OD to the ordinary circle curve dotted in,

R_1 = the radius OG, OE, OJ to the principal curve.

$R - R_1$ = DF = the normal distance between the circle curves named.

T = the tangent DC to the circle to radius R.

T_1 = the tangent AC to the new curve.

$T_1 - T$ = AD = difference of the two tangents.

i_1 = the angle between the tangent line to the easement curve at G, and the tangent T. i_1 = GOF.

D_A = total deflection angles laid off at A, from the tangent AC for running the easement curve. The greatest one for a particular curve is GAC.

D_l = total deflection angles at same point on the easement curve, from a line parallel to AC, to points beyond.

$D_{l=200}$ = total deflection angles for the instrument at 200 feet from A as measured along the easement curve.

l = length of the easement curve counting from A.

x_1 and y_1 = co-ordinates to the point G, as shown, but given for every 10 feet of the curve l.

From the fact that the easement curve AG is a certain definite spiral curve of increasing curvature, it is evident that it will fit all circle curves, GJ, of whatever radius; because, beginning with an infinite radius, it is only necessary to run it to where its radius equals that of the principal curve GJ, whatever that may be. Hence the various quantities pertaining to the easement may be calculated once for all for every point and tabulated. To do this we require equations, such for instance as are given below.

According to considerations already presented, we have

$$h = \frac{\text{const}}{\rho}$$

where h is the difference of elevation of the two rails, and ρ the radius of curvature of the spiral at any point. Also,

$$h = \text{const.}\, t^3 = \text{const.}\, n^3 = \text{const.}\, l^3 = \frac{\text{const}}{\rho} =$$

Take the constants such that

$$h = al^3$$

and

$$\rho h = \frac{a}{3b}$$

Then

$$\frac{1}{\rho} = 3bl^2$$

These are the fundamental relations.

Now at any point on the spiral easement curve the radius of curvature ρ is perpendicular to a tangent drawn to the same point of the curve; the latter, as above explained, making the angle i with the principal tangent T. Hence for a small variation of the position of the point considered, along the curve l by an infinitessimal dl, the radius ρ will swing through an infinitessimal angle di. Hence we have the relation

$$\rho di = dl,$$

or by introducing the value of ρ

$$di = 3bl^2 dl$$

Integrating this for limits reckoned from zero, we have

$$i = bl^3$$

Also by the figure it is easily seen that

$$\frac{dy}{dl} = \cos i = \cos bl^3,$$

$$\frac{dx}{dl} = \sin i = \sin bl^3$$

Expanding the sine and cosine into series, we have

$$\frac{dy}{dl}=1-\frac{(bl^2)^2}{1.2}+\frac{(bl^2)^4}{1.2.3.4}-\text{ \&c.},$$

$$\frac{dx}{dl}=bl^2-\frac{(bl^2)^3}{1.2.3}+\frac{(bl^2)^5}{1.2.3.4.5}-\text{ \&c.},$$

which, for limits reckoned from zero become

$$y=l\left(1-\frac{(bl^2)^2}{1.2.7}+\frac{(bl^2)^4}{1.2.3.4.13}-\text{\&c.}\right)$$

$$x=bl^4\left(\frac{1}{4}-\frac{(bl^2)^2}{1.2.3.10}+\frac{(bl^2)^4}{1.2.3.4.5.16}-\text{\&c.}\right)$$

From these equations, the co-ordinates to the spiral curve can be computed.

If we apply the subscript 1, to a particular set of quantities belonging to the point G in the figure, we may write

$$R_1=\rho_1=\frac{1}{3bl_1^2};$$

$$R-R_1=x_1-R_1(1-\cos i_1),$$

$$=x_1-\frac{1-\cos i_1}{3bl_1^2},$$

$$=x_1-\frac{bl^4}{6}\left(1-\frac{(bl^2)^2}{1.2}+\text{\&c.}\right);$$

$$T_1-T=y_1-R_1\sin i_1$$

$$=y_1-\frac{\sin i_1}{3bl_1^2}$$

$$=y_1-\frac{l_1}{3}+\frac{l(bl^2)^2}{18}\left(1-\frac{(bl^2)^2}{20}+\frac{(bl^2)^4}{873\frac{1}{3}}-\text{\&c.}\right)$$

For total deflection angles at A we have

$$\tan D_A=\frac{x}{y}$$

when x and y are co-ordinates to the point to be located by the angle D_A.

For deflection angles laid off at any point x' y' on the curve, from a line parallel to the tangent T, we have

$$\tan D_l=\frac{x-x'}{y-y'}$$

which applies for points forward or back x' y'. This deflection angle is useful when it is desirable to move the transit instrument from A to a point on the curve for passing obstacles, &c.

From a point on l, 200 feet from A, measured along the curve,

$$D_{l=200}=\frac{x-x_{200}}{y-y_{200}}$$

A deflection angle from the tangent T at any point y', on that tangent for locating points xy on the curve, is given by

$$\tan D_T=\frac{x}{y-y'}$$

These deflection angles are intended for use in the ordinary way in practice, along with the chain for running the curve.

The tangent T to the dotted curve is given in terms of the radius R of that curve, and the intersection angle I, by the well known relation

$$T=R\tan\tfrac{1}{2}I.$$

CONSTANT FOR PRACTICE.

For the elevation of the outer rail we have for 30 miles per hour of train speed, and for l in feet,

$$h=al^2=.0000793l^2 \text{ inches},$$
$$=.0000066l^2 \text{ feet}.$$

For 45 miles per hour, and l in feet.

$$h=a'l^2=.0001785l^2 \text{ inches},$$
$$=.0000149l^2 \text{ feet}.$$

The value of b which has been adopted is given by

$$\text{com. log } b=1.8955-10.$$

SPECIAL CASE OF EASEMENT CURVES ONLY.

That the whole curve may consist only of two equal portions of the easement curve tangent to each other in the middle, the points G and J must fall at E, and we must have

$$i_1=\tfrac{1}{2}I$$

also radius at E=radius for $i_1=\frac{1}{2}$ I or

$$R_1=\frac{1}{3bl^2}=\frac{l}{3i_1}=\frac{2l}{3bI}$$

where i or I is expressed in arc to radius unity, and common log $b=1.8955-10$.

The length of the entire curve is twice the length l_1 to the point where $i_1'=\frac{1}{2}$ I.

PATH OF CENTER OF CAR.

It has been explained that the center of gravity of the car is the point which

should describe the curve here laid down, and not the center point between the wheels. This requires that the track at the curve shall be laid outward of the line run by the instrument and chain, by an amount about equal at any point to the elevation of the outer rail; since the center of gravity of car and load is above the rails a distance about equal to the track guage.

THE FIELD PRACTICE.

To facilitate the field operations in running easement curves, values have been computed for every 10 feet of the curve and tabulated so that the curve may be staked out directly by stakes set 10 feet apart or at multiples of 10 feet. These computed quantities are given in the accompanying table, which the en-

TABLE FOR FACILITATING THE FIELD WORK OF EASEMENT CURVES.

$\frac{l}{10}$	R.	Degree for R.	R—R_1	R_1.	Degree for R_1.	T_1-T.	DA.	$D/_{l=200}$.	i_1.	El. ft. 30 M.	El. ft. 45 M.	x_1.	y_1.
1	424100	0° 0′	00	424100	0° 0′50″	6.67	0° 0′ 0″	....	0°00′ 1.6″	.001	.00	00	10
2	106025		..	106025	3 20	13.3	....	....	13.″	.003	.01	00	20
3	47124		..	47124	7 25	20.0	07″	....	44.			.001	30
4	26506	0° 13′	..	26506	0° 13′16″	26.7	25	....	1′44	.011	.03	.005	40
5	16964		..	16964	20 22	33.3	49	....	3 23			.012	50
6	11781	0 30	..	11781	29 18	40.0	1′26	....	5 50	.023	.05	.025	60
7	8656		.01	8656	39 45	46.7	2 18	....	9 18			.047	70
8	6626	0 52	03	6626	52 00	53.3	3 26	....	13 50	.042	.10	.080	80
9	5236		.05	5236	1 05 40	60.0	4 51	....	19 43			.127	90
10	4241	1 19	.07	4241	1 21 06	66.7	6 44	....	27 02	.065	.15	.196	100.
11	3504		.10	3504	1 38 09	73.3	9 03	....	35 58			.289	110
12	2945	1 53	.14	2945	1 56 46	80.0	11 41	....	46 43	.096	.21	.408	120
13	2508.		.19	2508	2 17 02	86.7	14 49	....	59 24			.560	130
14	2164.	2 39	.25	2164	2 38 56	93.3	18 23	....	1°14 10	.131	.29	.755	140
15	1884		.33	1884	3 02 30	100.0	22 56	....	1 31 14			1.000	150
16	1657	3 28	.43	1657	3 27 30	106.7	27 41	....	1 50 42	.172	.39	1.288	160
17	1468		.52	1468	3 54 22	113.3	32 58	....	2 12 54			1.630	170
18	1310.	4 22	.68	1309	4 22 30	120.0	39 25	....	2 37 42	.213	.48	2.064	180
19	1177		.84	1176	4 52 30	126 7	46 18		3 05 22			2.560	190.
20	1061	5 24	1.04	1060	5 24 23	133.3	54 02	△	3 36 18	.264	.59	3.144	199.9
21	963		1.26	962	5 27 42	140.0	1°02 34	3° 52 02″	4 10 18			3.820	209.9
22	878.	6 32	1.53	876	6 32 30	146.6	1 11 55	4 10 25	4 47 48	.321	.72	4.603	219.9
23	803.		1.82	801	7 09 28	153.3	1 22 34	4 29 25	5 28 48			5.500	229.8
24	738.	7 46	2.17	736	7 47 28	160.1	1 33 24	4 49 48	6 13 36	.385	.86	6.515	239.8
25	681		2.56	678	8 27 30	166.8	1 45 32	5 11 30	7 02 12			7.670	249.8
26	630	9 06	3.00	627	9 08 50	173.5	1 58 45	5 34 10	7 55 12	.451	1.01	8.974	259.7
27	585.		3.48	582	9 51 26	180.2	2 13 07	5 58 05	8 52 12			10.44	269.7
28	545	10 31	4.02	541	10 36 21	186.9	2 28 12	6 23 05	9 53 30	.523	1.18	12.06	279.6
29	509		4.63	504	11 23 17	193.5	2 44 22	6 50 25	10 59 18			13.84	289.3
30	476	12 02	5.30	471	12 11 14	200.1	3 02 19	7 19 10	12 09 42	.595	1.34	15.87	299.0
31	447		6.05	441	13 01 00	206.8	3 21 15	7 44 30	13 25 24			18.09	308.7
32	421	13 40	6.87	414	13 52 30	213.5	3 41 07	8 21 00	14 45 24	.676	1.52	20.51	318.5
33	397		7.75	389	14 46 08	220.2	4 02 30	8 54 00	16 11 12			23.18	328.1
34	376	15 15	8.71	367	15 40 30	226.9	4 25 20	9 28 60	17 43 00	.762	1.71	26.11	337.7
35	356.		9.87	346	16 37 04	233 6	4 49 10	10 03 30	19 19 00			29.27	347.1
36	338	17 00	11.00	327	17 35 05	240.4	5 14 23	10 41 22	21 01 00	.851	1.93	32.70	356.5
37	321		13.20	309	18 44 30	247 1	5 40 55	11 20 30	22 49 00			36.38	365.7
38	308.	18 41	13.45	294	19 35 00	253.8	6 08 34	12 00 30	24 43 00	.952	2.14	40.35	374.9
39	294		14.93	279	20 38 51	260.6	6 36 00	12 37 50	26 41 00			44.40	383.8
40	282	20 12	16.66	265	21 45 05	267.4	7 04 00	13 16 10	28 49 46	1.057	2.38	48.62	392.8

NOTE.—Difference between a 100 feet chord and its arc at 400 feet from A or for the lower line of table is 0.686 feet and it varies as the square of the degree of curve, and cube of the chord length.

The angle to the principal circle curve=$I-2i_1$.

The value of $I-2i_1$ can never be negative in practice. It equals zero when G and J fall at E in the figure.

gineer should have placed in his note book for convenient use in the field.

To illustrate the use of the table take the following:

EXAMPLE.

Given the intersection angle I=60° and the radius, R, for an ordinary circular curve=1061 feet.

Then by the usual formula and calculation for circular curves,

$T = R \tan \frac{1}{2} I = 1061. \tan 30° = 612.6$ ft.

Hence to run in a circular curve, we go 612.6 feet back on the tangent from the intersection point, and start with deflections and chaining, the total deflection having been made out.

But to introduce the easement curves we must go back from the intersection point the 612.6 feet, plus the tabular distance, $T_1 - T = 133.3$ found opposite R= 1061. or 612.6+133.3=745.9 feet=T_1; and from this point—A, in the figure—start with the chain and the total deflection angles given in the table according to the chord length. For 10 feet chords, setting stakes 10 feet apart, use all the deflection, D_A, given in the table. For 20 feet chords use alternate ones. For 50 feet chords use the 49″, 6′ 44″, 22′ 56″ and 54′ 02″. For any length of chord we must in this example end the easement curve at 200 feet, because by the table $\frac{l}{10}=20$, or $l=$ 200 where R=1061; and hence the last total deflection on the easement curve will be $D_A = 54' 02''$.

At this point the radius of the easement curve is $R_1=1060$ feet; and this is the radius of the principal, or circular curve extending it. The angle between the tangent to the easement curve at this point and the tangent T is $i_1=3° 36' 18''$, as given by the table. Hence the instrument can readily be set up at the end of the easement curve and brought to tangency. The circle may then be run, its deflection angle being half the degree of the curve or 2° 42′ 12″ as obtained from the table.

The length of the easement curve l, is 200 feet.

The angle of the principal curve will be $I - 2i_1 = 60° - 7° 12' 36'' = 52° 47' 24''$. This divided by the degree gives the number of chords of 100 feet, and consequently the length of curve.

If both easement curves have been run before setting the instrument at G, the work may be checked by sighting on J with the total deflection for that point.

The elevation of the outer rail for the principal curve is the same throughout as for the easement curve at G, and =.264 feet,=3.1″, for a 30-mile speed. For points along the easement curve, the elevation is given in the table.

These values of the elevation are the amounts by which to set the track outward in order to carry the center of gravity of the car on the curve as already explained. Hence the principal curve is to be laid outward about three inches, all its length. The easement curve is to be laid outward 0.2″ at 50 feet; 0.8″ at 100 feet; 1.8″ at 150 feet, and 3.1″ at 200 feet, where the circle curve begins. These are for the 30 mile speed, the offsets being found in the elevation column of the table.

II. SPEED AT GRADE CROSSINGS.

The so-called "know-nothing stop" appears to be in force everywhere at points where one track crosses another at grade. In some states this is obligatory by state law. But the practice is universal, and appears not to depend at all upon state law.

Very little thought appears to have been given to the subject of economical crossings of railroads. In some instances as much money appears to have been expended in cutting to make a crossing "*at grade*" as would have been required to fill sufficiently to put the crossing "above grade." But in many instances thousands of dollars more better have been expended to carry one line over the other, than to have placed them at grade.

Some roads will place their estimates of expenses for all their stoppages at a single crossing point at from 100 to 500 dollars per day. We will probably be entirely safe in basing figures on the lesser amount, as true, for a great number of railroads. For 300 days to the year, the $100 per day will pay interest at 6 per per cent. on an expenditure of half a million of dollars. Hence at such a point as the one now considered, it would be economy to make an expenditure of anything less than $500,000, to carry one line over the other. This money would

cut about a mile of tunnel. A hundred such grade crossings in a state would amount, on account of stoppages, to enough to build, equip and maintain a first-class railroad across the largest state east of the Mississippi.

But more definite figures on this point may be found of interest.

The forthcoming report of the Commissioner of Railroads of Ohio contains the following figures, viz.:

Total number of grade crossings reported by all roads in the State, 252.

Total miles of railroad, $5{,}835\frac{1}{2}$.

Average number of trains that passed over each mile of railway during the year, 5,680.

Gross earnings of all railroads in the State for the year 1881, $33,116,271.

From these figures we find the average distance between two consecutive crossings on any one line of road to be $\frac{5{,}835\frac{1}{2}}{252} = 23.1$ miles. Average number of trains over each mile in one day; counting 330 days to the year, Sunday being allowed as about a third of a day in train running, is $\frac{5{,}680}{330} = 17.03$. Gross earnings per day, $\frac{\$33{,}116{,}271}{330} = \$100{,}352$.

Assumiog the average distance run each day by one train, at 14.3 miles per hour, the time on the average required for a train to move from one crossing to the next, including all stops such as for taking and discharging local freights, taking water, stopping at crossings, &c., is $\frac{23.1}{14.3} = 1.61$ hours; or 96.6 minutes.

Now allowing five minutes as a fair average for the time lost by a train in making the crossing stop, we find that $\frac{5}{96.6}$, or 5.176 per cent. of the running time is consumed in stopping at grade crossings; time which, except for the crossing, would be used in making headway; because steam is up and all the needed men are at their posts of duty. The 5 minutes is taken as an average for all trains, freight and passenger; a figure which is placed considerably higher by some good judges. By avoiding this stop, it appears Ohio roads could increase their daily earnings by over 5 per cent of the actual earnings, or exactly to the amount $\frac{\$100{,}352}{1-.05176} = \$105{,}830$; which shows a gain of $\$105{,}830 - \$100{,}352 = \$5{,}478$ per day for Ohio roads; a gain in earnings which it is fair to suppose would follow the abolition of the know-nothing stop.

To find the cost of a single stop, we have by multiplying the average number of trains per day by the number of crossings reported $= 17.03 \times 252 = 4292.$ = the number of daily crossing stops. As these cost $5,478, it appears that a single stop costs as an average $1.28.

The total cost of stops for the year 1881 appears from the above figures to be $330 \times \$5{,}478 = 1{,}807{,}740$, or nearly two millions of dollars. This capitalized at 6 per cent., amounts to the enormous and seemingly incredible sum of over 30 millions of dollars. The actual number of crossings is evidently only half the number reported by all roads, because any one crossing gets reported by both of the roads intersecting. Hence the number of grade crossing-points in Ohio in 1881 is 126. It appears, therefore, that there might be invested on 6 per cent. borrowed capital at each crossing point the sum of $\frac{\$30{,}124{,}000}{126} = \$239{,}120$; or nearly a quarter of a million of dollars as the amount that might be expended at each crossing point for appliances which would enable trains to pass the crossings at full speed.

In some States the law compelling the know-nothing* stop has recently been repealed. This is true of Massachusetts and Ohio, but the repeal only followed convincing proofs that better systems for making the crossing existed. Switch and signal appliances have been so perfected of late as to place at the disposal of Railroad companies means for passing grade crossings at full speed in a manner conceded by those who are familiar with it to be decidedly safer than by the old compulsory *stop*.

To realize this fact of enhanced safety it should perhaps first be noted that the compulsory stop is not absolutely safe. For instance a freight train on a down

*Called the know-nothing stop from the fact of the passage of the law compelling it in Massachusetts the year of the political "know-nothings."

grade approach, might become unmanageable and break into a train making the crossing. A rear locomotive on a long freight train, especially when around a curve out of sight of crossing and flagman, might under certain circumstances remain under steam without knowledge of error, and push the forward end into a crossing train. Though such instances are rare, yet they are known to have occurred.

Suppose each branch of track at a crossing to be provided with a derailing switch, so that in each instance just named above, the train in error would have been derailed, or turned into a side track. This would have avoided the crash in the two instances mentioned, but the four switches, while avoiding two accidents, might occasion ten for the extra attention they require; unless accompanied by operating mechanism far superior in control to that which has been employed in past years. But the modern greatly improved and wonderfully perfect *interlocking* switch and signal apparatus is fully competent to the task.

Indeed the modern "block system," in making a single block each way at the crossing, would in all probability be as safe for passing at speed when clear, as would be the old-fashioned stop. But the addition of the derailing, or side-track switch on each branch of track, and so worked by interlock, with the signals of the block that only one track can possibly be set clear at a time, seems to leave nothing to be desired for absolute safety; at least for a far greater measure of safety than is possible with the old know-nothing stop.

Apparatus working with the degree of precision and certainty just indicated is already in use on some important lines of railway, a notable instance being found in the blocks by which the Pennsylvania Railroad enters the city from West Philadelphia to its magnificent new depot at Broad and Market Streets. Here all the switches for handling the 250 trains per day which are brought in and out of that depot, and the signals for governing the movements of those trains, are interlocked with each other. In one tower is a machine with 56 levers, and by it are operated all the switches and signals belonging to the track, extending from the depot back to a distance of about half a mile. By this machine all the trains can be handled at any one time by one man.

The most wonderful feature of all this maze of tracks, switches, signals, and operating rods, cranks and levers, is that they are so interlocked with each other that whenever the attendant (human and fallible), by inadvertence, siezes the wrong lever, he finds it locked. Thus he cannot set the signals to clear for a train to move until the switches are all in correct position. The breakage of an actuating rod leading to a signal would leave the signal to the action of gravity, and it is so made and weighted that it would fall to the danger position, and prevent the moving of the train until attended to. Inaction, incapacity or sleep of attendant simply causes delay. Signals not being cleared, trains are stopped.

Such appliances instated at crossings, would evidently provide safety next to absolute; and admit of the passing of trains at nearly, if not quite full speed—indeed at full speed when a rail-junction reversible frog for closing up the rail gaps shall come to be operated along with the derailing switches. Then no stops would be required at crossings except as two trains, at comparatively long intervals, would happen to require the crossing at nearly the same time. Then the signals and derailing switches would stand against that one which was a moment behind the other in announcing its arrival. It will then necessarily tarry till the first has passed, when the releasing of the "detector bar" will enable the man in the tower to turn the signals and switches just in use, back to the danger; thus unlocking the intersecting lines, switches and signals, so that the second train can be passed.

Rusty Bolts.—To remove bolts that have rusted in without breaking them, the most effectual remedy known is the application of petroleum. "Care must be taken that the petroleum shall reach the rusted parts, and some time must be allowed to give it a chance to penetrate beneath and soften the layer of rust before the attempt to remove the bolt is made."

THE STORAGE OF ENERGY.*

From "Nature."

THE subject of this lecture has been called by the world at large, even by well-informed *Punch*, "The Storage of Force." Why, then, have I ventured, in my title, to differ from so popular an authority? For this simple reason—that you cannot store force any more than you can store time. There is as much difference between force and work as there is between a mile and the speed of a train or between a ship and a voyage. Work involves two distinct ideas combined, whereas force only involves one. When a weight rests on the ground the weight pushes the ground down with a certain force, and the ground pushes the weight up with the same force. If, then, there were such a thing as a storage of force, the mere resting of a weight on the ground would be such a storage, since the force exerted between the weight and the ground never grows less. But, I need hardly say, it would be beyond the ability of the cleverest engineer to work a machine, or drive a train, by using a weight resting on the ground; the very expression, "dead weight," shows how useless it is for the practical purposes of producing motion. A weight resting on the safety valve of a steam engine may be a very good means of adjusting the pressure at which the valve shall open and liberate the excess steam, but this weight will never work the engine.

Work is force exerted through space; if a weight P be raised through F feet, $P \times F$ foot-pounds of work will be done, and there will be a store of $P \times F$ foot-pounds of work in the raised weight.

The continuous evaporation of the water from the seas and rivers by the heat of the sun, and its subsequent deposit in the form of rain on the hill tops, supplies us with another very large raised weight store of energy, and which is practically utilized when the water falling down the hill side works out water wheels and turbines.

Various stores of energy arise from the separation of two bodies which desire to come together. The vast fields of coal form an enormous store of energy, owing to the tendency of carbon to combine with oxygen. Copper which is found pure, and zinc, when separated from the oxygen with which it is combined in nature, are examples of the same kind. We may also have a store of energy arising from two bodies being too close together, and which desire to move apart; as, for example, in a coiled spring, in compressed gas, in two similar magnetic poles, or in two similarly electrified bodies near together.

The experiments now shown are examples of energy previously stored being utilized. This grindstone is being turned by a falling weight, the ventilating fan by falling water, this saw is worked by the gas engine, the lathe by this galvanic battery, and the sewing machine by three Faure accumulators.

The water which is falling from the top of the building, and which is working this turbine, was really stored in the cistern for drinking and washing purposes, and, although serving us as a store of energy, it was not pumped up for this purpose. Indeed the price charged for water by the water companies would prohibit its use for the production of power. For with water at a pressure of 100 feet, and at as low a price as 6d. per 1,000 gallons, it would cost 1s. 4d. per horse power per hour if the turbine had 80 per cent. efficiency.

In addition to the natural stores of water energy on our hill tops, there are also artificial stores of water energy, or Armstrong's water accumulators, as they are called, although invented long before Sir William Armstrong's time, and which are employed in many large steel works, docks, &c. Water is periodically pumped into a cylinder with a heavily-weighted piston, which is therefore raised when the water is pumped in. If then at any moment, at any part of the works power is required, a tap is opened, and this large weight falling at the reservoir cylinder, drives out the water and performs the desired piece of work.

Now I want to consider how far it

* Abstract of a lecture delivered at the London Institution, by Prof. W. E. Ayrton, F.R.S.

would be possible to drive a tramcar by one or other of these various sources of power. An ordinary tramcar for forty-six passengers weighed $2\frac{1}{2}$ tons, and when full of people about $4\frac{1}{2}$ tons. To pull such a car at the rate of six miles an hour along an ordinary line requires about $1\frac{1}{2}$ horse power. To produce such an amount of power for one hour requires an expenditure of over 2,800,000 foot-pounds of work, or if produced by a weight falling, say through 10 feet, would require the weight to be over 100 tons.

Armstrong's water accumulators are therefore clearly useless for the purpose, and coiled springs are too cumbersome.

Steam engines are occasionally employed on tram lines, and from the point of economy are much superior to horses; but there is the great disadvantage of the smoke, noise, and the terror of the horses of other vehicles. A detached tramway engine weighs as much as a full car, consequently nearly half the total horse power employed is used in propelling the engine and boiler, and there is also the waste of power caused by the rapid radiation of heat from the boiler of a small engine. Gas engines, though saving the weight of the boiler and coal, have the compensating disadvantage that per horse power, the weight of a gas engine is so much greater than that of a steam engine, and cannot therefore at present be economically employed for tram cars.

Compressed air engines have been employed with considerable success by Col. Beaumont for driving tram cars, and he has succeeded in storing in one cubic foot of air at 1,000 lbs. pressure per square inch enough energy to pull three tons about half a mile along an ordinary tramway. But successful as this system is from the point of economy, there is the same objection that there is to the steam tram, viz., the comparative great weight of the locomotive. The detached compressed air engine weighs about 7 tons, while the car full of passengers is hardly 5 tons, so that seven-twelfths of the total horse power expended is employed in pulling the compressed air engine alone. I understand it is proposed to build combined cars and compressed air engines, a change that will probably lead to a great improvement.

In order to obtain mechanical motion we require a store of energy, and some machine for converting the energy stored into mechanical work. Now experiment shows that the weight of an electric motor is but a small fraction of the weight of a small steam engine and boiler per horse-power developed. Electric motors, indeed, can be easily made to give out work at the rate of 1 horse-power per 50 lbs. dead weight of machine, and hence the great advantage of using them for movable machinery. (Experiment shown of drilling holes in thick wood with a hand electromotor and raising large boxes with a small electric hoist.) The most economical store of energy we can convert into mechanical energy by the agency of electricity is evidently the energy of coal, and this is the store we shall mainly employ in driving electric motors. That is to say, coal will be burnt to produce mechanical motion, the mechanical motion will work a magneto or dynamo electric machine to produce an electric current, the electric current, will be conveyed along the wires, and at the other end, by means of an electro-motor, the electric current will be reconverted into mechanical work. (Experiment shown.)

Instead of converting the electric current energy into mechanical motion I can convert it into heat, and I shall then have, as you see, the ordinary electric light.

But if the engine breaks down, the electric motor at the other end must stop, or the electric light go out; the constant occurrence of which accident has just decided the authorities at the Manchester Railway Station to discontinue the use of the electric light. To prevent this effect following such an accident, an electric accumulator is needed, that is a reservoir which has been drinking in the electric energy when the engine was going at its best, and which will now give it out when the engine has stopped. Again, apart from accidental fluctuations in the speed of the engine, or total breakings down there is another most important use for the electric accumulators. That the electric lighting of towns will become general, I need hardly to stop to prove to you, and that it will be carried out in ways quite different from the expedients temporarily

adopted is also equally obvious. But users of electricity in this country have at present to manufacture their electricity as they require it, and are in the same position that gas companies would be in if they were unable to store their gas, but had to manufacture it all while it was being consumed. They would evidently require much larger and consequently more expensive plant. Now the experience of two years has shown that, for large buildings, the electric light is far cheaper than gas. How much cheaper will it then become, when the electric energy can be manufactured at any time convenient, and stored until it is required to be used?

The earliest form of accumulator was simply a voltameter worked backwards. Now although Sir William Grove greatly increased the efficiency of this secondary battery by coating the plates with platinum black, still it was of little practical importance because of the rapid escape of the greater portion of the gases formed, if the charging was continued for a long time, as well as their diffusion through the liquid.

It is clear, then, we must arrange matters so that the passage of the primary current, forms on each plate a substance which has no tendency to wander over to the other. Such a substance must obviously be a solid, and a solid not soluble in the liquid. Now, an oxide of lead satisfies, in a marked degree, these conditions, and hence the employment in secondary batteries of this oxide, produced usually by sending an electric current between the lead plates immersed in dilute sulphuric acid.

But, in addition to having the plates near together, they must have large surface, in order to store much electric energy. And the way to give the plate a large surface, without making it inconveniently large, is to make it spongy. Hence what is aimed at is two spongy lead-plates near together.

Plante's method of accomplishing this occupied some months, and even when "well formed," his cell does not store very much electric energy, so that it has hardly ever been used for any commercial purpose.

In 1880, M. Faure thought of the device of putting a thick layer of red lead on his lead plates, a substance which can easily be reduced to spongy lead by the passage of a current. The plates, after being coated with red lead, are then wrapped in flannel jackets and put side by side in a box, every alternate plate being connected together, so as to practically produce two plates with very large surface very near together. To form the cells, reverse currents are sent somewhat in the same way that they are sent in forming the Plante cell, with the exception that only days and not months are required in the formation. The red lead on the one side is reduced to a spongy material, which is probably lead very slightly oxidized; on the other side, it is reduced to lead peroxide. Charging the cell, by sending a current in the direction of the last current sent, reduces the suboxide to pure lead, and the lead peroxide, on the other side, to an even more oxidized salt. On using the cell to produce an external useful current, the pure spongy lead becomes again slightly more oxidized, and the peroxide slightly less oxidized. In fact, there is a small quantity of oxygen which travels backwards and forwards as the cell is charged and discharged.

Now, does such a cell store electricity? No! emphatically no! When charging it, just as much electricity passes out as passes in, and, when discharging it, just as much electricity passes in as passes out.

Imagine a stream of water was turning a water-wheel, and the water-wheel was employed to raise corn up into a granary, the arrangement might be called one for storing corn, but certainly not one for storing water. So a secondary battery does not store electricity, but electric energy.

The pith, then, of Faure's discovery is the mechanical placing of a salt of lead on the leaden plates, the presence of which layer of lead salt enables spongy lead to be produced in a few days, instead of requiring many months, when the spongy lead is electrically formed out of the lead plates themselves by the long passage of electric currents.

The next point to consider is: (1) the storing capacity of such an accumulator; (2) its efficiency; (3) its durability. Now, I am glad to say, I am able to give you more than hearsay evidence on this point, since Prof. Perry and myself have been

engaged on rather a long series of experiments on this subject. I may mention that we were both rather sceptical about the merits of the Faure accumulator before commencing this investigation, since we feared that the reports of its excellent action were almost too good to be true. Our doubts, however, gradually dispelled themselves as the investigation proceeded, and we now are able to add our tribute to its practical value.

Let us take a single example of the storing capacity. A certain cell, containing 81 lbs. of lead and red lead, was charged and then discharged, the discharge lasting eighteen hours—six hours on three successive days; and it was found that the total discharge represented an amount of electric energy exceeding 1,440,000 foot lbs. of work. This is equivalent to 1 horse power for three-quarters of an hour, or 18,000 foot lbs. of work stored per lb. weight of lead and red lead. The large curve shows graphically the results of the discharge. Horizontal distances represent time in minutes, and vertical distances foot lbs. per minute of energy given out by the cell, and the area of the curve therefore the total work given out. On the second day we made it give out energy more rapidly than the first, and on the third more rapidly than on the second, this being done of course by diminishing the total resistance in circuit. During the last day we were discharging with a current of about 25 amperes. But in connection with the storing power, there is a very curious phenomenon to which I think not nearly sufficient attention has been directed, and that is the resuscitating power of a Faure's cell. When a cell has been apparently completely discharged, and is left for a few hours by itself, it appears to have obtained a new charge. For example, after the eighteen hours' discharge just referred to, although there apparently was no electric energy left in the cell at the end, it was found that after a few hours' insulation, the accumulator could give a current of over 50 amperes, and produce therefore bright flashes of fire. The phenomenon is wonderfully like the invigorating action of sleep. In one case, during our experiments of an extremely rapid and powerful discharge, we found that in subsequent discharges after rest, the cell gave out three times as much energy as it did in the first discharge. The neglect of considering this resuscitating power has doubtless misled many people who have possibly discharged a Faure's cell very rapidly into under estimating its storing capacity.

Secondly, as regards efficiency. The efficiency of an electric accumulator—that is, the ratio of the work put into it to the work given out—depends on the speed with which it is charged, and the speed with which it is discharged. If charged or discharged too quickly, a certain amount of energy will be wasted, heating the cell itself; since, whenever a current passes through a body, some heat is developed, and the greater the current the greater the heat, the latter indeed increasing much more rapidly than the current. Now, it is possible, in a way I will not at the moment trouble you by explaining, to distinguish between the work given to the cell to produce chemical decomposition and the work wasted by too hurried charging. Similarly, in discharging. it is also possible to find out how much of the electric energy stored up in the cell is wasted in heating it by too hurried discharging. Allowing for such unnecessary waste, experiment shows that, for a million foot-pounds of stored energy discharged with a mean current of 17 amperes, the loss in charging and discharging combined need not exceed 18 per cent.; indeed, in some cases, for very slow discharges, we have found it not to exceed 10 per cent. I do not, of course, mean by this, as some people have mistakenly imagined from the published numbers of Prof. Perry and myself, that a current of only 17 amperes can be obtained by discharging a single cell; since, of course, far greater discharge-currents can be produced if the external resistance be low; indeed, I shall show you a constant discharge of about 70 amperes presently. In speaking of the number 17, I merely mean to say that was the average current when the experiments on the efficiency above referred to were made.

As to deterioration, two months constant charging and discharging of the two test-cells showed no signs of deterioration.

I have said that a cell containing 81 lbs. of lead and red lead stored 1,440,000 foot-pounds of work. Now, consider what

that means. It represents all the energy required to be expended to pull a tramcar containing forty-six passengers over two miles, after allowing for considerable waste of power in the electrical arrangements. The electromotor and gearing need not weigh, as I told you, more than about 200 lbs., to produce about two horse power. We have, therefore, this wonderful conclusion, that about 300 lbs. dead weight contains all the energy and all the machinery necessary for over two miles' run of a tramcar with forty-six passengers. Now, is this result actually obtained at present in the tramcar running at Leytonstone, and which is propelled by Faure's accumulators? No, and why? Partly because the electro motor has not been made to suit the accumulators, nor the accumulators the electromotor. nor is the gearing adapted to either.

The cells, as at present made, would not give off their energy quickly enough; hence a greater number are employed, but which, consequently, require to be charged much less frequently than would otherwise be necessary. Indeed, in a ton of the cells as at present constructed, there is about fifty miles' run of a tramcar containing forty-six passengers.

But, in spite of the temporary character of this arrangement, the total weight of the Faure cells, dynamo and gearing combined, used at Leytonstone, is only $1\frac{1}{2}$ tons, or one-third of the weight of a detached steam or compressed air engine commonly used for tramcars.

Spacious as is the Lecture Theater of the London Institution, it is unfortunately not large enough to admit a tramcar. I have therefore done the next best thing to prove to you that the Faure accumulators really contain a vast store of available energy. We have here a circular saw which is now cutting wood over an inch in thickness. As you see, the circular saw is driven by that Gramme electromotor, and the electromotor itself is fed by the energy stored up in these accumulators, and which was put into them by a dynamo machine yesterday, on the other side of London.

When the Faure's accumulator was first invented, there were various suggestions of electricity being delivered at houses every morning like milk in cans, and the exaggeration of this idea no doubt did something to prejudice the cells in the eyes of the public. The reason why milk is delivered in cans and brought by carts is simply because the total quantity required is so extremely small. If milk were required to be consumed in large quantities like water is, we should have it sent through pipes, and not by cans. The main use of the accumulators will be as stationary reservoirs corresponding with cisterns for water or gasometers for gas. But in certain cases where the accumulators can be used to propel a cart, as in the case of tramcars, not the cart employed solely to carry the accumulators, then there is not the same objection to their being moved about, seeing that the total weight necessary is small compared with that necessary for a steam-engine or a compressed air engine for tram lines to develop the same horse power.

Again, just as ordinary electromotors are not made to discharge a Faure's cell rapidly, so ordinary electric lamps are unsuited for this purpose; and, therefore, although there is enough energy in a 100 lbs. dead weight of Faure accumulator, to give a light of 1,500 candles for thirty minutes, an ordinary electric lamp cannot be illuminated at all by a single ceil. Mr. Edison, however, has been turning his attention to this subject, and here is the result of his handiwork, which arrived last night from America, and which is, therefore, shown for the first time in England this evening. This incandescent lamp, as you see, only requires four Faure accumulators to illuminate it, this one eight, and this other one twelve. But must the accumulators be even as large as those I am using on the table? The answer is, No; if you do not require them to give out the light for a very long time. Four much smaller boxes would give just as much light as you see at the present moment; but, of course, would not keep the light burning so long. It is, therefore, now possible to have a box of accumulators and an incandescent lamp, and the whole thing quite easily carried by one man.

Last year Prof. Perry drew attention, in his lecture at the Society of Arts on the "Future of Electrical Appliances," to the great waste of energy that is produced by the coal being carried to the steam engine, instead of steam engines being brought to the coal, and the power given out by

the engines conveyed electrically to the place where it was commercially required. Why, said he, should not the coal be burnt at the pit's mouth, or in the pit, or even in that part of the mine where the seams were thickest, and the engines driven by burning it used to work large dynamo machines on the spot, and the power transmitted electrically to any towns where it was required? Again, it has been often asked, why should not the wasted power in streams be utilized? At present it is more economical to use steam engines in a town than to do work in the country by means of the streams, and convey the manufactured articles over the hills into the towns; and for that reason one sees the old water-wheels, in the neighborhood of a place like Sheffield, being gradually deserted, and the men preferring to pay a higher rent for steam-driven grindstones in the town, to a smaller rent for water-driven grindstones in the suburbs. The question then arises would it be possible to convey economically the power from the coal pits or from the streams into the towns by means of electricity; and this obviously turns on, how much power can be got out of one end of a wire compared with the amount that is put in at the other? I have, during this evening, been talking of the measurements of electric energy put into or taken out of an accumulator of foot-pounds, and you may have wondered how it was possible to measure electric energy in the engineer's unit of foot-pounds. In reality it is very simple. The maximum amount of work a waterfall can do, depends on two things, the current of water and the height of the fall. In the same way, the work a galvanic cell or accumulator can do, depends on two things, the current it is producing, and what is called its electromotive force, the latter being analogous with the difference of pressure or head of water. Again, when electric energy is being turned into mechanical work by means of an electromotor, the energy which is being put into the motor can be measured by the product of the current sent through the motor, and the electromotive force maintained between the terminals of the motor. Now, here are two instruments, devised by Prof. Perry and myself, an Am meter and a Volt meter, the one for measuring a strong current, and the other a large electro-motive force. With these we will now make simultaneous measurements when we allow this motor, which is driving the lathe, and which is itself driven by an electric current, to run at different speeds. First, we will start with the motor, which has one ohm resistance absolutely at rest, by putting a brake on it, and ending by allowing it to run as fast as possible.

Experiment performed and the following results were obtained:

Speed of motor.	Current in Amperes.	Electromotive force between terminals of the motor in volts.	Electric power put into the motor in foot-pounds per minute.	Power wasted by the current heating of the wires of the motor in foot-pounds per minute.
0	15	15	$15\times15\times44.25$ *i.e.* 9956.25	$15^2\times1\times44.25$ *i.e.* 9956.25
Slow	10	21	$10\times21\times44.25$ *i.e.* 9292. 5	$10^2\times1\times44.25$ *i.e.* 4425
Fast	4	28	$4\times28\times44.25$ *i.e.* 4956	$4^2\times1\times44.25$ *i.e.* 708

We see in the last case, when the load was light and the speed of the motor very great, there was less than one-tenth of the waste of power arising from the current heating the wires when the speed was very slow. On the other hand, we observe that the electromotive force between the terminals of the motor has been practically doubled.

This simple experiment really points to the solution of economic transmission of power by electricity, and to which Prof. Perry and myself have on numerous occasions directed attention. It is, to allow only a very small current to pass through the wires connecting the electro-motor with the generator, and to maintain a very great electro-motive force between them; since, in this way, the amount of power transmitted can be made as large as we like, and the waste from the heating of the wires from the passage of the current as small as we like.

Reasoning in this way, Sir W. Thomson showed, in his inaugural address last year to the British Association, that, if we desire to transmit 26,250 horse-power by a copper wire half an inch in diameter, from Niagara to New York, which is about 300 miles distance, and if we desire not

to lose more than one-fifth of the whole amount of work—that is to deliver up in New York 21,000 horse-power—the electromotive force between the two wires must be 80,000 volts. Now, what are we to do with this enormous electromotive force at the New York end of the wires? Fancy a servant dusting a wire having this enormous electromotive force. You might as well, as far as her peace of mind is concerned, ask her to put a lightning flash tidy.

The solution of this problem was also given by Sir W. Thomson on the same occasion, and it consists in using large numbers of accumulators. All that is necessary to do in order to subdivide this enormous electromotive into what may be called small commercial electromotive forces is to keep a Faure battery of 40,000 cells always charged direct. from the main current, and apply a methodical system of removing sets of 50 and placing them on the town supply circuits, while other sets of 50 are being regularly introduced into the main circuit that is being charged. Of course this removal does not mean bodily removal of the cells, but merely disconnecting the wires. It is probable that this employment of secondary batteries will be of great importance, since it overcomes the last difficulty in the economical electrical transmission of power over long distances.

I will conclude my lecture by illustrating one of the other important uses to which the accumulator can be applied, and that is the practical lighting of railway trains, which may be seen in daily operation in the Pullman cars on the Brighton line. The most natural method of lighting a railway train would be to attach a dynamo-machine to the axle of one of the carriages—the guard's van, for example—and the rotation of which, necessarily very rapid when the train is going fast, would, without the use of any gearing, produce the necessary current. But the difficulty that immediately meets us is that as soon as the train slows, or stops at a station, or in consequence of the signal being against it, the speed of the dynamo-machine will diminish and the lights will go out. If, however, while the train is going fast, the dynamo performs two operations, the one to keep the lights burning, the other to charge a battery of Faure's accumulators on the train, then the electric energy so stored can be applied to maintain the lights while the train is going slowly or stopping. With such an arrangement there would be, of course, an automatic contrivance for disconnecting the dynamo-machine from the circuit when the speed becomes too low; otherwise the Faure's accumulators would simply discharge themselves back through the dynamo-machine.

Imagine, now, we are in a train which is going slowly, or which has actually stopped, and that the Faure accumulators lying here on the floor is the Faure battery in the train, and which has been charged when the train was going fast; then that it has sufficient store of energy to continue lighting is proved, because, on connecting these two wires, those fifty Maxim lamps, kindly lent me by the Electric Light and Power Company, and eight Edison lamps before you, are instantly brilliantly illuminated, each of the former possessing about forty candle-power, and each of the latter about seventeen, and giving, therefore, far more light than is at present ever supplied to a whole train of twelve carriages. The light, you observe, is perfectly steady, and is turned on and off at will. Imagine, now, we are in a tunnel in the daytime, and the lights, therefore, burning. We now emerge from the tunnel into daylight. I disconnect the wires, and the lights are instantly extinguished. Again, it may be, we are entering a second tunnel. The wires are again connected by the guard, and we have the whole of this lecture theater, which represents, the train, brilliantly illuminated.

There has been an erroneous impression existing lately, that the Faure accumulator could not produce a constant current of more than 17 amperes; but that this is a mistake is clearly seen from the fact, that at the present moment, each of the cells in this room is producing a current of about 75 amperes.

Electric storage of energy, therefore, makes us nearly independent of accidents to the engine or dynamo machine, or irregularities in their working, enables us to receive our supply of electric energy from the central supply station in our proper turn, and independently of the particular time we require to utilize it, and lastly it enables large amounts of power to be transmitted over very long distances with but little waste.

ON THE FORMATION OF SAND BANKS AND SAND HILLS, AND THE CONSTRUCTION OF HARBORS ON SANDY COASTS.

By H. KELLER.

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THE author holds that all coast lines are in a continual state of change from the action of the sea, the rate of variation being slower as the materials of the land are more resisting, and the force of the waves less great. The general effect is to wear down promontories and fill up bays; but to this there are many exceptions: thus the point of Dungeness has advanced 90 yards in fifty-two years. Such cases are due to the action of special currents, combined with low wave power.

Sandy Coast.—The rock and earth of the cliffs, after being shaken down by the breakers, are by the same cause ground into smaller and smaller fragments, till they arrive at the state of sand. The fragments are roughly sorted, according to weight, by the carrying power of the waves, and when they have reached a depth too great for direct wave action, the finer portions are still moved by the currents. By studying the geological character of the shingle and sand at various points of a coast, the direction of its drift, and consequently that of the prevailing currents, can generally be determined.

Flat coasts, especially of the tertiary and quaternary formations, are the chief localities of sand, owing both to the large area which is acted on between high and low water, and to the large horizontal motion of waves in shallow water. At the water's edge, where the waves are finally spent, a flat and even strand is formed; further down, where the advancing and retiring waves meet in conflict, the sand is violently agitated, and heaped up into ridges, while during each movement it is carried onwards for a short distance by the set of the prevailing currents. These currents, and the sand they transport, pass straight across the mouths of narrow inlets or bays, and thus form bars or sand banks, which often convert the latter into lagoons. Similarly a row of islets may be connected with each other, and with the mainland, by accumulations derived from such currents. The quantity of sand thus transported, on any given sandy coast, cannot easily be estimated. Where harbors are choked by it, dredging operations, though useful in the case of shingle, are of no permanent avail, in consequence of the inexhaustible supply of sand furnished by a long coast line; and no operations for cutting off this supply are of much effect.

Influence of River Silt.—The mud and sand brought down by rivers add of course to the accumulation of sand banks, though much of it is so fine as to be carried at once into deep water. The amount of this addition does not depend so much on the quantity brought down as on the coarseness of its quality, and the effects of winds and currents at the river mouth in causing it to settle near or far from shore. The shingle is of course deposited first, then the sand, and lastly the mud.

Breadth of Quicksands.—Various observations seem to show that the zone of quicksand, *i. e.*, of sand continually in motion, does not extend below the point at which the direct or indirect action of the waves ceases; its breadth is therefore in general small.

Formation of Sand Banks.—Wherever a current charged with silt has its speed seriously reduced, deposition may take place. The cause of such reduction may be the meeting with an obstacle, such as an island or wreck, the meeting with another current, a change of direction, &c. Of these the second is the most important; the same cause which makes the sand banks prevents their rising into islands, and they often become very large. The two currents may be both ocean currents, due to temperature, or one an ocean current and the other the outflow from a river.

Effect of Storms.—Were the weather always equable, the changes of a coast line would be very slow, depending only on the erosion of land by the sea and by the rivers, and on the shifting of the sand by currents; storms, and "storm floods," by which is meant the heaping up of the sea against the coast in heavy landward gales, have, however, a very great and disastrous effect in breaking up sandy shores and sweeping away the materials. The rounded outline of the east coast of England, as compared with the deeply-indented coast line of Friesland, is evidence in itself that in the German Ocean the prevailing gales are from the west. Such washing away of the coast may be assisted by geological causes, such as the yielding nature of the strata, or a secular sinking of the land. The result is the retreat of the land in most places, often accompanied by an advance elsewhere, where the materials washed away are deposited. In many places the latter has been largely assisted by human enterprise in the way of reclamation.

Coast Currents.—The main causes of ocean currents are differences of temperature; but these differences are greatly lessened in the neighborhood of land. Apart from special local currents, such as those flowing out of inland seas, the main cause of powerful coast currents, such as move sand and shingle, is the wind. Such currents usually change their direction as the wind shifts, and the general drift of the sand is in the direction of the prevailing winds. The energy of waves driven by such winds against the shore in an oblique direction is expended partly in heat and erosion, but mainly in moving the water, sand, &c., partly up and partly along the beach. The latter movement is the greater, as the wind is more oblique to the coast line. Coast currents thus formed have in some cases a speed of 6 feet per second, and extend to a depth of 30 feet, and it is these irregular currents which mainly cause the movements of sand and the formation of sand banks.

In regions where the range of tide is considerable, the currents of ebb and flow add another important factor to the causes of sand movement. They sometimes assist and sometimes oppose the effects of wind currents and temperature currents, and have the greatest influence on shelving coasts, where that of the waves is less than on flat coasts. The periods of maximum velocity, both of ebb and flow, the duration of each, &c., are very much influenced by the peculiarities of different seas and estuaries, and must be studied separately for each case.

Formation of Sandhills.—When the wind blows nearly perpendicularly on a sandy coast it stirs up the dry sand, and drives it onwards in successive bounds. Where the sand is stopped by natural or artificial obstacles sandhills accumulate, which may be formed into regular chains of "dunes." If an oblique wind from the sea blows upon such dunes, it disturbs their seaward face (unless it be properly planted or fascined), and drives the sand partly inland over the top, partly along the face. In this manner thick clouds of sand often travel along the coast, and sometimes choke up the mouths of streams, &c. Where there are openings in the foremost dunes, the sand rushes through, and forms other dunes further inland. The sand of such dunes is thus continually traveling, both along the coast and inland—an evil which can only be checked by planting the dunes with vegetation, and by continual care. In some cases complicated systems of dunes are built up by local causes, and form sandy wastes of great extent. The opposite effect, viz., the blowing of sand into the sea by seaward winds, is not usually of much importance.

Action of Engineering Works on the Coast-line.—The object of such works is either the warding off of dangerous currents, or the causing sand to accumulate at particular places, or the protection of harbors. The first are only required in places where the coast-line is in an unstable condition, as at the mouths of rivers. The second, such as groynes, are intended to form deposits, as it were, of sand, which may eventually check the drift of sand under the action of coast currents. They can only be very partial in their operation, unless they are distributed over the whole length of coast under treatment. Piers, projected into the sea to protect harbors against the incursions of sand, are generally acknowl-

edged to be only of temporary advantage; since the sand gradually works its way round them, even when they are carried forward beyond the depth at which coast currents usually operate. Exceptions to this rule only occur where some of the causes of sand movement happen to be absent.

The direction which such piers should take is not fully established. In reclamation works on rivers, a slight inclination against the current is known to be best; but for harbor piers a perpendicular direction may sometimes be preferable. The object should be to divert the sand moving along the shore into deep water outside the harbor, by curves as easy as possible, and allow it afterwards to return to its general direction. The angle between the pier and the coast always forms a sort of bay, in which the waves tend to pile themselves up, and a reflux is thus produced, which cuts out a deep hollow along the pier. The sand entering the angle is carried outwards by this reflux, until it meets at right angles the main coast current, which has been little influenced by the pier. At this point the speed is checked, and sand deposited, which gradually forms a shoal in the line of prolongation of the pier. This shoal shelters the water between it and the pier, and favors the deposit of sand there; so that eventually a compact sandbank is formed round the head of the pier, and extending some distance in front of it. For these reasons an inclination in the direction of the current seems the best. The heaping up in the angle is then less, and the sand comes out at an angle to the coast current, and mingling with it is carried forward without settling. This will be facilitated if the shape of the pier is made convex towards the current, which at the same time leaves the shore behind it quite open to the sweep of the seas, and assists the transport of the sand into deep water. Whatever form is adopted, such harbors will, however, always require a great deal of dredging inside. The reason is twofold; first, that the set of the flood tide usually diverts the coast current into the mouth of such harbors, and deposits the sand in the still water; secondly, that in storms the waves fling masses of sand-laden water into the harbor, with the same result.

Thus at Boulogne the shoal of La Bassure lies off the mouth of the harbor, and leaves between itself and the coast a narrow and deep channel, to which the shore falls in terraces. The Atlantic tide-wave, coming in from the west, causes strong currents along this channel in alternate directions; and since the new piers, now building, will be carried out into this channel, it is hoped that these currents will keep the entrance always open, although dredging will no doubt be required within the harbor. On the other hand, the harbor of Ymuiden, lately constructed with two piers inclined towards each other, after the model of Kingstown, already shows signs of shoaling near the entrance.

Action of Scouring Currents on the Coast-line. — By a scouring current (*Spulenstrom*) is meant any current (generally that from a river or estuary) which prevents the formation of sand-banks by scouring them away as they are deposited. Where the current is due to a river, its effects will be greatly influenced by the amount of silt it carries of itself, which may even turn it from a scouring to a depositing current. Where it comes from an estuary it is generally clear, because the estuary forms a settling basin, in which the silt is deposited. In some cases the current may be due to the reflux of the waters driven into a lagoon by the wind; but such entrances, unless under very rare circumstances, can never be permanent.

In the two former cases the scouring is continuous, but varies greatly in intensity with the time of the year, height of tide, &c. The direct effect of a current of clear water is to drive outwards the coast current and the sand it carries, which is gradually deposited in the form of a concave bar round the mouth of the river. This is usually cut through in one or more places by narrow channels, its form, &c., depending on the relative action of the fresh water, the coast current, and the prevailing wind. The outer side of this bar is acted upon by the waves, and when there is a gale full on them the sands on this side are stirred up, and carried over to the inner face of the bar, or even into the harbor. By this means the bar may sometimes be increased in height, and moved towards the harbor, in spite of the fresh water efflux. This

efflux can often be concentrated, and so made more effectual, by the construction of piers.

Currents entering the estuary from the sea bring in silt, which is deposited where the current dies away, *i. e.*, in rivers at the upper limit of the tide, and in lagoons at the inner end of the connecting channels, which thus gradually silt up.

Where the upland waters are not clear, but carry silt and shingle, things are altered. The former mainly passes at once into deep water; the latter settles first on the inner bar just described, until a flood carries the whole of this bar into the sea, where it goes to increase the outer bar. This bar, gradually rising on each side of the river channel, may contract it so much that it may finally be diverted, thus illustrating the formation of deltas.

Action of the Tide in Estuaries.—When the tidal wave is checked by entrance into an inlet or estuary, its forward edge becomes higher and steeper; and where the rise of the bottom is rapid, the depth small, and other circumstances intervene, the regular form of the wave is lost, and it rushes upwards as a "bore." In the case of lagoons, the tide advances more quietly, and generally deposits a good deal of silt; occasionally the ebb leaves the lagoon by a different channel from that by which the flood has entered.

The flood tide, pouring into an estuary, brings with it sand and mud, of which part at least is deposited where the velocity comes to an end. Hence the tidal area of a river is a sort of reservoir of silt, which oscillates up and down till it either sinks permanently to the bottom or is swept out to sea on the ebb. In sheltered places sand banks and islands are thus formed. The same tends to take place outside the mouth of the river; but then such sand banks, after having grown to a certain extent, always come under the action of the coast and other currents, and are cut back again. The formation of sand banks or deltas within the estuary, as described, tends to form the same accumulations outside, because it diminishes the tidal capacity of the estuary, and therefore the power of the ebb to scour these sand banks away. The final result must be the filling up of the whole of the tidal basin, outside the actual low-water channels, and the conversion of extended estuaries into flat marshes, cut by deep and narrow streams. These will often find their way into the ocean by several mouths, especially when they carry much silt, and are subject to violent floods, causing them frequently to break open new channels. Where, from such causes, an estuary falls below its required width and depth, artificial works become necessary. The object of such works should be to keep the energy and volume of the ebb and flow as great as possible at every part, and at every time. The fall, section, sectional area, and discharge of a stream are all dependent on each other; hence, if the discharge be increased, the fall and section will in general increase also, and, if care is taken that the banks are not attacked, the channel will be deepened, An estuary, however, comprises two different parts—the tidal channel within the river and the basin at the mouth—and these require different treatment. Parallel training banks are the right method in the former, while in the latter the object should be to cut off subsidiary channels, and to concentrate the flow.

Similar considerations apply to the case of lagoons, which must in time either be filled up entirely, or converted into lakes, separated from the sea by banks of shingle and sand.

Harbor Bars.—In the formation of harbor bars, two forces besides the tide are concerned, viz., the prevailing wind and the coast current. Much depends on the angle which the direction of these make with each other. Where wind and tide meet full against each other, the result is a stoppage of velocity and consequent deposition of silt, combined with a violent agitation or surf at the surface. The bar is thus rendered doubly dangerous. The depth of the entrance will in general be greater (as examples show) the more inclined it is to the direction of the waves. Hence the entrances of rivers, in a stormy sea, are seen to take a direction more and more inclined towards the coast, until at last the mouth gets choked by the action of some storm, and the river then breaks a new way straight through the bar. For this reason breakwaters should be made convex to the

direction of the wind, so as to give an oblique direction to the current issuing from the harbor.

The author then treats of the construction of harbors on sandy coasts.

Maintenance of Depth in Harbors.—Most harbors on sandy coasts owe the maintenance of their depth solely to the scouring action of the estuary which forms them. They are usually divided into an inner harbor or dock, and an outer harbor, often connected by a half-tide basin. The outer harbor may be a natural reach of river, as at Newcastle, or an artificial basin. On sandy coasts this basin will in general be in a continual state of silting up, and a bar will be continually forming in front of it, as already shown. All apparent exceptions to this rule are either on large and powerful rivers or on rocky coasts. The interior of the basin can be easily kept clean by dredging, but the dredging of the bar is a different matter.

For cleansing the interior, in cases where the range of tide is great, artificial scour is often resorted to. The water, either tidal or upland, is impounded in a basin, and let out through sluices towards low water. As the issuing stream has first to put the whole water of the basin in motion, it is some time before it reaches its maximum velocity, and this period should be made to coincide with that of dead low water. The silting up of the scouring reservoir itself is often a difficulty, which has not been successfully met by admitting only the upper and clearer layers of the tidal water. If fresh water is used, rubbish, logs of wood, &c., are collected in the scouring basin, and eventually deposited on the bar. The effect of such scour does not reach below a depth of 6 to 9 feet, so that its power upon a bar is limited. It is also inconvenient to the ships using the harbor, and apt to undermine foundations, &c. This may be obviated by placing the sluices outside the half-tide basin, leaving the latter to be cleansed by dredging. The effect of scouring the harbor entrance itself has not been fully tried, but works for this purpose are in course of erection at Calais and Honfleur. In such harbors the piers are generally so long that it is impossible to reach their outer ends by scouring from within (natural or artificial), unless the resistance to the scour is unusually small. To make it act with effect on the bar, the pier should be made concave to the scour, which will run round it and then radiate outwards to the place required. This is preferable to training the current by low-water walls, which impede the entrance and cause surf. Movable training pontoons, moored in the tideway before scouring, have been employed, but should only be used for old harbors, where a permanent pier cannot be had. A much better mode of increasing the scouring effect is to bend the channel as nearly parallel as may be to the direction of the waves and currents, as described above. In general, with the view of assisting the scour, all sharp turns, sudden changes of section, and trumpet-shaped entrances should be avoided, as these tend to weaken the action of the current.

Action of Scour.—Lentz gives 0.75 meter (2½ feet) per second as the lowest velocity that will scour silt, and 1.50 to 2 meters (5 feet to 6½ feet) as the lowest that will scour sand. These are nearly ten times as great as the corresponding values given by Dubuat, &c., for river water; but the explanation is that the former refer to the power of raising and scouring away, the latter to the power of transporting merely. Thus the first of a series of scouring always has the best effect, because it acts upon silt which has only lately settled, and is easy to move. Hence it comes that the artificial scour is rarely useful at any great distance from the sluices, because the velocity is lost in causing eddies, and in putting the surrounding masses of water in motion. The remedy is to put the scouring basins right at the mouth of the harbor, as mentioned above. To this the objections are, the expense, and the fear of damage by storms. To avoid this it has been proposed by Bouquet de la Grye to lay pipes, or a masonry culvert, from the scouring basin along the pier, with sluices at intervals, opening upon the entrance. Another suggestion is that of Bergeron,* to lay pipes along the bottom to the bar itself, and use hydraulic pressure to stir up the sand, which would then be carried away by the ebb tide. The trials of this promising method have not been suc

* *Vide* Minutes of Proceedings Inst. C.E., vol. iii. p. 132.

cessful, and the possibility of using it in bad weather is very doubtful. Another method also suggested by Bergeron, is the use of vacuum dredgers, removing the sand by suction, which work well even in bad weather. These and other mechanical means should only be considered as accessories to the scour, assisting its erosion by forming a channel for it. This has been done at Honfleur by planting a row of piles, or preferably of buoys moored on to the bottom, which, being agitated by the current, form eddies and stir up the sand.

When artificial scour is employed, it generally takes place only at spring tides. The sluices are opened a little before low-water, and the scouring lasts one and a half to two hours. This rarity of action has a bad effect, as compared with continuous natural scour, owing to the opportunity given to the silt to settle and harden. Moreover, the natural scour of the ebb, which at least keeps the silt in suspension, should be taken advantage of. Artificial scour should therefore be more frequent, begin earlier, and continue till the turn of the tide. Difficulties in the way of this can be met by the same means as before suggested, viz., by making the discharge basins and the sluices close to the entrance.

Arrangements of Harbors with regard to Winds and Waves.—In many harbors the easy keeping open of the entrance is of less moment than the protection given from the sea, and the means of safe entrance in all weathers. On rocky coasts and in wide bays the works required for this purpose are generally simple, and consist in removing obstructions such as rocks, and building breakwaters to shelter the whole or a part of the bay from the prevailing winds. Where no bay exists, a harbor can be formed by the building out of two piers, with or without a breakwater in front of the mouth. These piers should not have salient corners, and should be convex, not concave, towards the sea. The harbor should widen rapidly within the entrance, so that the waves may spread out and be lost, and vessels be at once in safety. In designing the entrance, the needs of vessels entering are of course to be considered much more than those of vessels leaving, especially in the case of harbors of refuge. This does not apply so much to harbors on flat sandy coasts, as the depth at low water is usually too small to enable them to be used as harbors of refuge. Here it is not so much storms which have to be considered as the prevailing wind; and the entrance should be so placed that vessels can make it without sailing at an angle of more than 60° at the outside to this wind. To lay the entrance directly in line with this wind is not advisable. It is quite unnecessary for sailing vessels, especially in these days of steam tugs; the vessels entering come too rapidly and those leaving are greatly impeded, while the harbor is exposed to the full run of the waves, and the scouring power of the ebb is much reduced. Trumpet-shaped entrances have also this last disadvantage, and increase instead of diminishing the violence of the waves. Whether the two piers should be of unequal length must be decided by local circumstances; in general the best arrangement seems to be that the pier next the prevailing wind should be shorter than the other, as this facilitates the entrance of vessels. The entrance should not be, if possible, perpendicular to the coast current, as it is then harder to make, especially by long vessels.

Artificial harbors have sometimes been made with two entrances, but this is objectional. In some cases a single breakwater has been built across the mouth of a bay, with an entrance in the middle; but this gives rise to bad cross seas between the impinging and reflected waves. The outer ends of the piers should be inclined towards each other at an angle of about 90°, but not so as to be in a straight line. The entrance should never be exactly opposite the quarter of the heaviest gales. This especially applies if the outer harbor is to be used for unloading goods. When the entrance is long and narrow, it is generally curved gradually away from the direction of the storms. The curve must be very gentle if it is to accommodate the long ocean steamers of the present day.

It is often impossible to attain to all the above advantages, especially in channel harbors, as opposed to artificial basins. In the former the waves are sometimes broken up to some extent by inter-

posing jetties of open pile work, with side basins behind them. It has been found advantageous to make the piers themselves open above low water. On the Tyne and elsewhere the mouth of the estuary has been partly closed by piers, thus forming a sort of basin behind them. This, from its preventing the free ingress of the tide, will probably lead to silting up near to the mouth, though in the case of the Tyne the immense dredging operations higher up tend to remove this difficulty.

After recapitulating the conclusions arrived at, the paper gives a general project for a tidal harbor on a sandy coast. The points of first importance are protection against waves, convenience of scouring, and prevention of excessive accumulations of the sand traveling along the coast in the direction of the prevailing winds. The pier exposed to this sand must be long and convex, thus inclosing a sort of basin within it. This should be turned into a scouring basin by means of an inner pier run out from the shore with a slight curve to meet the other or windward pier close to the entrance. At the point of meeting the sluices will be placed. Between this third pier and the leeward pier will be the entrance to the inner harbor, which will thus have a channel form. The third pier may be pierced by a number of openings, closed on the ebb but open on the flood, which will tend to dissipate the waves as they enter the harbor, during the time of high water, when the traffic is heaviest. The entrance will be inclined as much as possible to the prevailing wind, and the scouring operations will take place on every tide, and be continued as long as possible, so as to hinder the silt from settling, or stir it up before it has become compact. By such means the bar continually formed by the advance of the sand will be as continually swept away into deeper water. While the construction of these works will no doubt be costly, the depth will thus be permanently preserved at the least possible cost.

The paper contains sixteen plans o harbors, &c., and a great number of references to particular cases, which for the sake of brevity have been omitted.

THE THEORY OF THE GAS ENGINE.

From "English Mechanic and World of Science."

At the meeting of the Institution of Civil Engineers held last week, a paper by Mr. Dugald Clerk was read, "On the theory of the Gas Engine." The practical problem of the conversion of heat into mechanical work had been partially solved by the steam engine; but its efficiency was so low that it could not be considered as complete or final. Hot air in the past had been looked upon as a possible advance. Owing, however, to many futile attempts, it had long been deemed useless to look in that direction for better results. The great progress made in recent years with the gas engine, from the state of an interesting but troublesome toy to a practical powerful rival of the steam engine, had shown that air might, after all, be the chief motive power of the future. Three distinct types of gas engines have been proposed:

1. An engine drawing into the cylinder gas and air at atmospheric pressure for a portion of its stroke, cutting off communication with the outer atmosphere, and immediately igniting the mixture, the piston being pushed forward by the pressure of the ignited gases during the remainder of the stroke. The instroke discharged the products of combustion.

2. An engine in which a mixture of gas and air was drawn into a pump, and was discharged by the return stroke into a reservoir in a state of compression. From the reservoir the mixture entered a cylinder, being ignited as it entered, and without rise in pressure, but simply increased in volume, and following the piston as it moved forward, the return stroke discharged the products of combustion.

3. An engine in which a mixture of

gas and air was compressed or introduced under compression into a cylinder, or space at the end of a cylinder, and then ignited while the volume remained constant and the pressure rose. Under this pressure the piston moved forward and the return stroke discharged the exhaust.

Types 1 and 3 were explosion engines, the volume of the mixture remaining constant while the pressure increased. Type 2 was a gradual combustion engine, in which the pressure was constant but the volume increased. Calculating the power to be obtained from each of these methods, supposing no loss of heat to the cylinder, it was found that an engine of type 1 using 100 heat units, would convert 21 units into mechanical work; in type 2, 36 units; and in type 3, 45 units. The great advantage of compression was clearly seen by the simple operation of compressing before heating, the last engine giving for the same expenditure of heat 2.1 times as much work as the first. In any gas engine, compressing before ignition, igniting at constant volume and expanding to the same volume as before ignition, the possible duty D was determined by the atmospheric absolute temperature T′, and the absolute temperature after compresssion, T; and it was $D = T - T' \mid T$, whatever might be the maximum temperature after ignition. Increasing the temperature of ignition increased the power of the engine, but did not cause the conversion of a greater portion of heat into work. That was, the possible duty of the engine was determined solely by the amount of compression before ignition. Compression made it possible to obtain from heated air a great amount of work with but a small movement of piston, the smaller volume giving greater pressures and thus rendering the power developed more mechanically available. Seeing the great difference produced between types 1 and 3 by the simple difference in the cycle operation when there was no loss of heat through the sides of the cylinder, the question arose, Which engine in actual practice, with the engine kept cold by water, would come nearest this theory? In which of the engines would there be the smaller loss of heat? Comparing the two engines, with equal movements of piston, it was found that the compression engine had the advantage of a lower average temperature and a greater amount of work done; also of less surface exposed to flame, and consequently it lost less heat to the cylinder. Taking all the circumstances into consideration, it was certainly not over-estimating the advantages of the compression engine to say, that it would, under practical conditions, give for a certain amount of heat three times the work it was possible to get from an engine using no compression.

It was interesting to calculate the amounts of gas required by the three types under the supposed conditions. Taking the amount of heat evolved by one cubic foot of average coal gas as equivalent to 505,000 foot-pounds, and calculating the gas required if all the heat were converted into work, it was found to be 3.92 cubic feet per H.P. per hour. Therefore, the amounts of gas required by the three types of engine would be:—

Type 1. $\frac{3.92}{0.21} = 18.3$ cubic ft. per HP. per hr.

" 2. $\frac{3.92}{0.36} = 10.9$ " " "

" 3. $\frac{3.92}{0.45} = 8.6$ " " "

Comparing these figures with results obtained in practice from the three types of engines losing heat through the sides of the cylinder, it was ascertained that the amount of gas consumed was as follows:—

Type 1. Lenoir, 95 c. ft. per I.H.P. per hr.
Hugon, 85 " " "
" 2. Brayton, 50 " " "
" 3. Otto, " " "

It would be seen that the order of consumption was what was required by theory. The Otto engine converted about 18 per cent. of the heat used by it into work, while the Hugon engine only converted 3.9 per cent. Taking the loss of heat to the cylinder, as given by the comparison of the adiabatic line of fall of temperature with the actual line of fall as shown on the indicator diagram, it appeared much less than really was the case, as shown by the gas consumed by the engine. The maximum pressure produced was much less than would be ex-

pected from the amount of gas present; this was due to the limiting effect of chemical dissociation. The gas engine presented a more complicated problem than a hot-air engine using air heated to the same degree. Analyzing the disposal of 100 heat-units by Clerk's gas-engine, it was found to convert 17.8 into work, to discharge 29.3 with the exhaust gases, and to lose to the sides of the cylinder and piston 52.9 units. About one half of the whole heat used passed through the cylinder and heating water. St. Claire Deville had shown that water was decomposed into its constituents at a comparatively low temperature, considerable decomposition taking place at 1.200° Centigrade. The cause of so near an approach to the line of theoretical fall, as was shown by the actual indicator diagram, was simply the continuous combination of the dissociated gases. At a maximum temperature of about 1,600° Centigrade, complete combination of the gases with oxygen was impossible, and could only take place when the temperature fell low enough.

In calculating the efficiency of the gas engine from its diagram, all previous observers had fallen into error, through neglecting the effects of dissociation, and, accordingly, their results were much too high. To account for this so-called sustained pressure, Mr. Otto had advanced the theory that inflammation was not complete when the maximum pressure was attained at the beginning of the stroke, but that by a peculiar arrangement of strata he had made it gradual, and continued the spread of the flame while the piston moved forward. Mr. Otto called it slow combustion. This designation seemed to the author to be erroneous; such an action should rather be called slow inflammation. It existed in the Otto engine, but only when it was working badly, and was attended with great loss of heat and power. This was proved by a diagram, and by certain considerations deduced from Bunsen and Mallard's experiments on the rates of propagation of flame through combustible mixtures. The conclusion arrived at was that slow inflammation was to be avoided in the gas engine, and that every effort should be made to secure complete inflammation at the beginning of the stroke. The author had found it possible to ignite a whole mass in any given time, between the limits of one-tenth and one-hundredth part of a second, by arranging the plan of ignition so that some mechanical disturbance by the entering flame was permitted. A diagram taken from the Otto and Langen free-piston engine, as given in a paper by Mr. F. W. Crossley, and an analysis of his reasoning, showed that the results were misinterpreted, and false conclusions arrived at concerning the nature of an explosion. Mr. Crossley considered that an explosion of gas and air, pure and simple, must be accompanied by a rapid rise and an almost instantaneous fall of pressure. This, he thought, was proved by the diagram, but in this statement the author could not concur.

From the considerations advanced in this paper, it would be seen that the cause of the comparative efficiency of the modern gas engines over the old Lenoir and Hugon type was to be summed up in the one word "compression." Without compression before ignition an engine could not be produced giving power economically and with small bulk. The mixture used might be diluted, air might be introduced in front of gas and air, or an elaborate system of stratification might be adopted, but without compression no good effect would be produced. The gas engine was, as yet, in its infancy, and many long years of work were necessary before it could rank with the steam engine in capacity for all manner of uses. The time would come when factories, railways and ships would be driven by gas engines as efficient as any steam engines, and much safer and more economical of fuel. The steam engine converted so small an amount of the heat used by it into work that, although it was the glory and the honor of the first half of this century, it should be a standing reproach to engineers and scientists of the present time, having constantly before them the researches of Mayer and Joule.

THE boring of the Arlberg tunnel is proceeding rapidly, the rate of advance averaging ten meters daily which exceeds the average made with the St. Gothard by six meters. At this rate boring is expected to be completed before the end of 1883.

REPORTS OF ENGINEERING SOCIETIES.

ENGINEERS' CLUB OF PHILADELPHIA.—Record of Business Meeting, May 6th, 1882.

The memorial to Congress of the American Metrological Society, asking for the adoption of means by which a common meridian might be established for the reckoning of longitudes and local time, was presented and unanimously approved. The pamphlet from the American Society of Civil Engineers, upon Standard Time for the United States, Canada and Mexico, accompanied by questions to interested persons with regard to the various propositions, was presented and discussed.

The objects set forth in House Bill number H. R. 4726, were unanimously approved and a Committee appointed to transmit to our Members of Congress the sentiment of the Club upon this subject, and to take such action as might best further the interest in this Bill.

Mr. Russell Thayer exhibited a section of an underground conduit for electric light, telegraph or telephone wires.

Description.—This conduit consists of a box or pipe made of terra cotta, artificial stone or porous earthenware (in sections) glazed on the outside and saturated with paraffine or crude petroleum. (In the sample the paraffine is not properly introduced, it should be saturated into the pores of the material in a liquid state while the material is warm and the paraffine melted. The conduit should not simply be coated with paraffine.) The box is made in two parts divided horizontally, the upper portion serving as a lid or cover to the lower part, and the lower part is constructed with grooves or depressions running longitudinally, for the reception of the wires. The sections are placed in the ground and joined and cemented together with rings, and laid like an ordinary terra cotta pipe.

Advantages.—This form of conduit possesses the following advantages, viz.: it is very inexpensive and very durable, indeed permanent in its character. It is easily made and can be laid by ordinary laborers. Being made in two parts (an upper and a lower) there is no difficulty whatever in placing the wires in it, and if a wire should from any cause become damaged or be defective at any points in the conduit, it is entirely accessible, since the cover can readily be removed from any section, the wire will be repaired and the cover be replaced. The wires do not have to be pulled or forced through a long tube or pipe as has been done heretofore. Electric light or telegraph wires already placed on poles, can be transferred to this conduit without breaking the circuit or disturbing the current for a moment, since being made in two parts, the conduit can be placed in the ground, the wires be transferred thereto, the lid be placed thereon, and the trench be filled and the street be repaved as fast as the pipe or conduit is laid. This is obviously impossible to perform with a continuous pipe, tubes or arrangements of that description.

It can be constructed of any reasonable size to hold any number of wires, and the wires are completely insulated from each other by the paraffine or crude petroleum with which the material of the conduit is saturated. The saturating material also prevents the entrance of water or moisture into the conduit. A patent for this conduit has been applied for.

Mr. Thayer also presented the following:

While the subject of the construction of new bridges across the Schuylkill river is being considered by Councils, I desire to record an observation relative to their design which I think could, with advantage, be considered. It is simply this. There appears to be no good reason why the bridges built across this stream should be raised to such a great elevation above the water level. At their present elevation the bridges are a complete obstruction to the passage of ships that cannot lower their masts; and it certainly seems to me that any new structures that are built could be lowered considerably and at the same time not interfere with the traffic on the river any more than at present. The only change necessary would be that the tugs and steamers would be obliged to hinge their stacks so that they could be lowered while passing under the arches. Some of the most celebrated stone bridges in the world, viz.: those constructed by the French engineers across the Seine at Paris, are almost all low structures, with the roadway nearly level transversely, and their stability and beauty of architectural effect have caused them to become models for similar structures in all parts of the world. The advantages of constructing bridges in the manner suggested are apparent, and may be briefly stated as follows, viz.:

1. Economy.
2. Greater stability.
3. Better approaches.

Economy.—Because less masonry is required.

Greater Stability.—Because there would be less weight bearing upon the foundations from the piers; and also because if there is any horizontal or oblique resultant of force tending to push the pier out of the vertical, the level arm of said resultant in a low pier is much less than of a high one.

Better Approaches.—Because from the configuration of the ground on either bank of the river, the grades are more suitable for a low bridge than for a high one. As at present constructed, the grades on either sides of the bridges are very steep, and when the pavements are slippery they are almost unscalable. Now, were the bridges not raised so high above the water, the roadways over them would be a much more easy gradient. Indeed, it seems to me, that they might with advantage be made quite flat; not, however, on a dead level, as I think a *slight* rise in the center of the structure is desirable, on account of drainage and architectural effect.

I have briefly referred to this subject as the matter seems to be one of interest at the present time, and if new bridges are to be built, that design should be adopted which, considering all conditions and requirements, would be the best for the locality in question.

May 20th, 1882.

Vice-President Percival Roberts, Jr., in the chair.

Mr. T. M. Cleemann read a paper on the "Most Economical Height of Bridge Truss." He said that in most cases of bridge design, after the span was fixed, the height of the truss was only governed by the judgment of the engineer, who generally assumed a proportion derived from some previously constructed bridge. It is not difficult, however, to find the most economical height, and the method applied to a Howe bridge was explained, and the result of a similar application to one of the largest iron bridges heretofore constructed likewise stated.

He also continued some remarks that he had previously made on the strength of wrought iron columns, especially discussing certain experiments which had been lately made at Watertown, with the formulas that had been proposed to represent their strength.

The latter paper was discussed at some length by Messrs. H. Constable, Strong, Haupt and P. Roberts, Jr.

Mr. Geo. S. Strong gave an interesting illustrated description of experiment in the application of his Feed Water Heater to locomotive engines, and also described new devices of his invention, for the piston and connecting rods of locomotives and for a spark arrester.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—The Annual Convention of the Society was held at Washington, May 16th and 19th.

The principal papers read were—

An Instance of Zymotic Disease in Metals. By O. E. Michaelis.

Subaqueous Underpinning. By A. G. Menocal.

Overflow of the Mississippi River. By Lyman Bridges.

The Hudson River Tunnel. By Wm. Sooy Smith.

Other papers presented but not read for want of time were—

Experiments on the Flow of Water. By A. Yteley and F. P. Stearns.

Targets for Rifle Ranges. By O. E. Michaelis.

Accuracy of Measurement as increased by repetition. By S. S. Haight.

Highway Bridges. By James Owen.

The following important reports of committees previously appointed were read and discussed:

Upon a Uniform System of Tests of Cements.

Upon the Preservation of Timber.

The address of President Welch delivered on the 16th we shall reprint in the August issue of this Magazine.

ENGINEERING NOTES.

THE BRIDGE ACROSS THE FIRTH OF FORTH.—The Select Committee of the House of Commons has passed the bill authorizing the construction of a bridge across the Firth of Forth at Queensferry, with the stipulation that the bridge is to be constructed under the superintendence of an officer appointed by the Board of Trade. The proposed new bridge is in substitution of the one sanctioned in 1873, according to the designs of the late Sir Thomas Bouch, inasmuch as it will be a steel-girder bridge, instead of a suspension bridge, while in strength and stiffness it is calculated to sustain a rolling-road three times greater and a wind pressure five times greater than was at first intended. The substituted bridge has been designed by Mr. Fowler, C.E., assisted by Mr. T. E. Harrison, chief engineer of the North-Eastern Railway, and Mr. Barlow, chief engineer of the Midland Railway, whose plans have been submitted to a committee of the Board of Trade, consisting of Col. Yolland, General Hutchinson and Major Marindin, who are satisfied with the provisions made as regards strength and stability. The bridge, which is almost a mile in length, will consist of two central spans of 1,700 feet and two side spans of 675 feet, approached on each side with spans varying from 115 feet to 150 feet. The clear height above high water is to be 150 feet for a width of 500 feet at the center of each 1,700 feet opening, and is intended to carry a double line of rails throughout. The cost of the construction is estimated at £1,730,000, and the time allowed in the bill for its completion is limited to five years.—*Iron.*

THE SAHARA INLAND SEA.—The French Government have recently bestowed greater attention upon the project, which has been before the public for several years, of connecting the depression of Rharsa and Melrirh, in the Northern Sahara, by a sea canal with the Mediterranean. The basin in question, probably a dried-up salt lake, has an elevation much lower than the level of the Mediterranean, the depression being in some places as much as 165 feet below that level. It is proposed to admit the sea-water into this natural basin, which covers a surface seventeen times the area of the Lake of Geneva, by a canal, starting from the Bay of Gabes, 33 feet deep and 330 feet wide, of a total length of 150 miles. In order to reduce the heavy expense attaching to the construction of such a canal, it is to be made at first of smaller dimensions, leaving the remaining work to be done by the flow of water. The benefits which France will derive from such a work are evident. It is expected that the canal and the inland sea would favorably change the climate of that terribly sterile region, improve French trade with Algeria and the Soudan, and confine the hostile irruptions of the Sahara tribes. But serious apprehensions are felt as to the success of the undertaking, which has been planned by Major Rondaire. It is especially feared that, on account of defective circulation, the process of evaporation would involve a constant inflow from the Mediterranean, which would soon surcharge the new inland sea with salty matter, and in that case destroy all existing organic life, thus converting it into another Dead Sea The French Government, in order to arrive at a true solution of the problem, have appoint

ed a commission charged with thoroughly investigating the question of this inland sea. Its report will be looked forward to by all interested in the matter.

IRON AND STEEL NOTES.

EXPERIMENTS ON THE STRENGTH OF WROUGHT IRON AND STEEL AT HIGH TEMPERATURES. By C. R. Roelker.—This paper contains no original matter, but is an interesting summary of previous investigations. Kollmann's experiments at Oberhausen included tests of the tensile strength of iron and steel at temperatures ranging between 70 and 2,000 degrees Fahrenheit, and the mode of conducting these tests is detailed in the paper. Three kinds of metal were tested, viz., fibrous iron having an ultimate tensile strength of 52,464 lbs., an elastic strength of 38,280 lbs., and an elongation of 17.5 per cent.; fine grained iron having for the same elements values of 56,892 lbs , 39,113 lbs., and 20 per cent.; and Bessemer steel having values of 84,826 lbs., 55,029 lbs., and 14 5 per cent. The mean ultimate tensile strength of each material expressed in per centum of that at ordinary atmospheric temperature is given in the following table, the fifth column of which exhibits, for purposes of comparison, the results of experiments carried on by a committee of the Franklin Institute in the years 1832–36.

Temp. Fahr. °	Fibrous Wrought Iron. Per cent.	Fine grained Iron. Per cent.	Bessemer Steel. Per cent.	Franklin Institute. Per cent.
0	100.0	100.0	100.0	96.0
100	100.0	100.0	100.0	102.0
200	100.0	100.0	100.0	103.0
300	97.0	100.0	100.0	106.0
400	95 5	100.0	100.0	106.0
500	92.5	98.5	98.5	104.0
600	88.5	95.5	92.0	99 5
700	81.5	90.0	68 0	92.5
800	67.5	77.5	44.0	75.5
900	44.5	51.5	36.5	53.5
1000	26.0	36.0	31.0	36.0
1100	20.0	30.5	26.5	—
1200	18.0	28.0	22.0	—
1300	16.5	23.0	18.0	—
1400	13.5	19.0	15.0	—
1500	10.0	15.5	12 0	—
1600	7.0	12.5	10.0	—
1700	5.5	10.5	8.5	—
1800	4.5	8.5	7.5	—
1900	3.5	7.0	6.5	—
2000	3.5	5.0	5.0	—

Comparing Kollmann's results with those of Fairbairn, Styffe, and the British Admiralty, and the author finds that the former differ from the latter in respect of there being found no increase of strength at temperatures higher than the ordinary atmospheric temperatures.—*Proceedings Inst. Civil Engineers.*

CORROSIVE EFFECTS OF STEEL ON IRON IN SALT WATER.—This paper read before the Naval Architects by Mr. J. Farquarson, detailed an experiment designed to ascertain the relative corrosion of iron and steel, and the corrosive effect on these of the combination when immersed in sea water. Plates of iron and steel of equal size, with an aggregate surface of 48 superficial feet, were used. After having the scale completely removed by dilute hydrochloric acid, they were singly weighed, marked, and placed in a grooved wooden frame, parallel and 1 inch apart, iron and steel alternately. The first, third, and fifth pairs were electrically combined by straps of iron at the tops; the second, fourth, and sixth pairs being left unconnected, and therefore each plate of which was only subject to ordinary corrosion, as if no other metal existed. The whole series so arranged were placed in Portsmouth Harbor, and left undisturbed for six months, when they were taken up and again weighed. The loss of each plate was found to be as under:—

		Oz.	Grains.
Steel	combined	0	427
Iron	combined	7	417
Steel		3	340
Iron		3	327
Steel	combined	0	297
Iron	combined	7	77
Steel		4	0
Iron		3	190
Steel	combined	2	337
Iron	combined	6	0
Steel		4	157
Iron		4	57

From the above it will be seen that the three iron plates combined with steel lost 21 oz. 57 grs.; that the three similar iron plates not combined lost only 11 oz. 137 grs. The plates were identical in size and all cut from the same sheet, the effect of combination with steel being to nearly double the loss of weight. The proof that the great excess of loss was not due to anything in the places themselves will be clearly seen by comparing the combined and uncombined steel plates, thus:—The three combined with iron lost only 4 oz. 187 grs.; the three uncombined lost 12 oz. 60 grs., or nearly three times as much as those protected electrically by the iron.

STEEL PLATES FOR BOILERS.—In 1879 the French congress of engineers refrained from pronouncing definitely on the relative value of steel and iron plates for boilers, being of opinion that the question was not then ripe for decision. The fifth congress, which recently met at Lyons, has once more inquired into the subject, and has submitted, according to the *Bulletin* of the Association parisienne des Proprietaires d'Appareils a vapeur, the following report:—Two boilers ordered by the Midi Company of the Fives-Lille Works burst at the trial, and the company consequently decided not to use steel plates, notwithstanding that Creusot offered every guarantee for its boilers. The Forges et Chantiers de la Mediterranee have likewise excluded steel plates from boilers. Krupp has also given up steel, and the experiments made at the instance of the English Admiralty have shown that steel corrodes more quickly than iron. This corrosion is all the more dangerous, as steel plates are used much thinner than iron plates. Mr. Webb, of Crewe, not-

withstanding, still adheres to the application of steel plates for the engines of the North-Western Railway. The engineers of Rouen also employ steel plates, on the ground, presumably, that they would prove more homogeneous in case of overheating. But this advantage is, according to M. Roland, of too small account compared with the great drawback that they are very liable to tear and burst at the ends and in the rivet holes either during manufacture or during use. He cites in support of his views the case of the eight boilers made by Messrs. Elder and Co. for the Livadia, of which three burst at a pressure of 3½ to 6½ tons per square inch, the result being the rejection of all the boilers. M. Cornut expressed the prevailing opinion of the congress when he stated that at present steel plates do not offer sufficient safety for the construction of steel boilers, and that it would be advisable not to employ them. He assumes that an amount of care would be required in the manufacture of steel used for this purpose which few makers would be inclined to exercise, and that to this circumstance must be ascribed the many failures observed in this department of the use of steel.—*Iron.*

ORDNANCE AND NAVAL.

SUBMARINE WARFARE.—Engineering science is still actively engaged upon devising means for the most rapid and effectual destruction of an adversary in naval warfare. A new submarine torpedo boat, the invention of M. Dgevetsky, has recently been tried at Kronstadt. It is a very small boat, about 20 feet in length, and weighs, when fully equipped, not quite two tons. The boat has the form of a cigar; its screw propellor is moved by the feet of four men placed in the central part of the vessel beneath a small glass dome through which the officer in command can see the submerged portion of the enemy's vessel, and accordingly direct the attack. The speed attainable by this boat is four miles an hour, which, it is considered, is amply sufficient to enable a subaqueous attack to be made upon vessels lying at anchor or approaching. The steerage of the boat presents no difficulty. To lower it to the distance of 50 feet and to raise it again to the surface of the water is rendered an easy operation by a very ingenious device. This elevation or depression is effected by means of weights made to slide upon longitudinal, horizontal bars or guide rails. When the boat is fully stored, charged and equipped, its normal position is just beneath the surface of the water, the upper portion of the glass dome alone slightly emerging. When it is desired to sink to a certain depth, the weights are slid forward to the prow of the boat, which, upon the propellor being set in motion, immediately begins to descend. The depths attained are shown by a specially constructed manometer. As soon as the boat has reached the desired depth, the weight is moved back to the center of the boat, and the latter now takes a horizontal direction. In order to rise to the surface, the weight is slid back to the stern, and thus an upward direction is communicated to the motion of the boat. Each of these boats is provided with a couple of mines or torpedoes, attached to it by means of levers. As soon as the boat passes underneath an enemy's ship, these can be instantly detached, and are so constructed as to mount upwards, and, by means of a gutta-percha appliance, attach themselves pneumatically to the enemy's hull. The attacking boat then retires to a safe distance, paying out at the same time the electrode wires in connection with the torpedo, which is then exploded. A supply of air compressed to a 50th of its normal volume is kept in a strong reservoir for the inhalation of the crew manœvering the subaqueous vessel, and is emitted by valves of a particular construction. Sufficient air is stored in this way to last 24 hours, and the exhaled gases are at the same time absorbed by chemical means.

THE NORDENFELT TORPEDO BOAT.—Another very formidable weapon in naval warfare, and similar to the torpedo boat of M. Dgevetsky, but differently manœuvered, is the new submarine vessel of Herr T. Nordenfelt (the inventor of the gun which bears his name), which was recently launched at Karlsvik, near Stockholm His boat is also cigar-shaped, oxposing, when floating on the surface, only a tortoise like deck with a copula—of glass, we suppose—just large enough to hold the head of the commander. Her dimensions are: Length, 64 feet; height in engine room, 7½ feet; whilst the engines of 100-horse power will, it has been calculated, propel her for short distances at a speed of 15 knots, and, when under water, at a speed of 12 to 13 miles an hour. The weight of the vessel, with machinery, coals and full equipment, is 60 tons. When attacking an enemy, the boat approaches to within striking range, descends a foot under the surface, and by the course determined before she descends, and by instruments indicating exactly how far she has proceeded, and to what depth she has gone, she may approach near enough to catch the shadow of the vessel intended to be destroyed, when the torpedoes are fired at the vessel's bottom. When under water, the boat is fully protected against fire, and when on a level with the surface, the cupola—18 inches in height—alone offers a target, almost indistinguishable among the waves, even at short distances. She will be armed with two fish torpedoes, propelled by compressed air, and also fitted with two rocket torpedoes for defence or attack at short distances. She is likewise provided with a crane by which the water ballast in the vessel can be quickly shifted, when she is not in motion, or if the automatic apparatus should get out of order. She is managed by three men, who can without difficulty spend several hours under water, and who are to this end provided with air bags attached to the back which supply air through an indiarubber feeder. The greatest safety for the crew consists, however, in the circumstance that the vessel floats on the surface until the machinery for sinking her and that for keeping her under water commences working; and consequently should part of her machinery be-

come damaged or cease working, she will at once shoot up to the surface, an action which can be further accelerated by the discharge in a couple of minutes of the entire water ballast of six tons. She is also constructed with four water-tight compartments, which will prevent her from sinking before reaching the surface at all events, thus giving the crew, provided with life-saving apparatus, an opportunity of escaping. The vessel has been built entirely of soft Swedish steel ½ inch to $\frac{5}{8}$ inch in thickness, and she is therefore stronger than the ordinary torpedo boat, which generally has but $\frac{1}{8}$-inch plates. Experiments will be made at Stockholm shortly, when every precaution will be taken until her thorough safety has been ascertained. The first trial of descending under water is to be made in a dock, whilst the crew, provided with diving costumes, will be in communication with the shore by telephone. The vessel has, we understand, been built at the expense of Herr Nordenfelt. For several years attempts have been made in different countries to construct such marine war vessels, but the greatest difficulty encountered appears to have been quickly to control the movements of the vessel, and also to keep the men, without danger, under water for any length of time. The first of these problems appears to have been successfully solved in this vessel, as she possesses a horizontal as well as vertical steering apparatus, the latter being automatic, so that the vessel's equilibrium in water is fully controlled by hydraulic machinery.

RAILWAY NOTES.

GROWTH OF THE AMERICAN RAILWAY SYSTEM.—The growth of the Railway system of the United States is one of the most remarkable items in the entire field of industrial statistics. The 8th of October, 1829, may be called the birthday of the railway system, as having been the day on which the locomotive trials were commenced at Rain Hill, on the Liverpool and Manchester railway. The earliest year for which we have official returns of the length of English railways is 1854, at the close of which 8,053 miles of line had been completed in the United Kingdom. In 1830 twenty-three miles of railway were open in the United States. By the end of 1840, 2 818 miles were open. In 1850 the length rose to 9,021. In 1854 it was a little more than double the length of the English lines, being 16,720 miles. By 1860 the aggregate rose to 30,635 miles against 10,433 in the United Kingdom. In 1870 the respective lengths were 52,914 and 15,537, and at the end of 1879, 82,223, and 17,696 miles respectively. The total length of the railroads of the United States at the close of 1880, including some lines which do not report their earnings, was 93,671 miles.

It thus appears that if we compare the growth of the railroad system since 1854 in the United Kingdom and in the United States, there has been a steady increase in the former at about the rate of 3 per cent., and in the latter at about that of 4½ per cent. per annum. But when we consider, not length of line alone, but length and cost together, the contrast is more remarkable. The lowest cost per mile of an average English railway is that shown by the returns for 1866, in which year the cost per mile of line open was £32,840. From that date the cost of the railways of the United Kingdom has steadily increased, till, in 1880, they have cost £40,613 per mile open. The American railways, on the contrary, have decreased their costliness, the average cost of a mile open in 1871 being nearly £12,000, and in 1880 only about £11,600. The total capital returned as expended in 1880 was £979,500,000 in the United States, and £,802,000,000 in the United Kingdom. The average gross earnings of the American lines was £1,460 per mile, of which 41.4 per cent. was net revenue. The United Kingdom lines averaged nearly £3,700 per mile of gross earnings, of which between 48 and 49 per cent. was net revenue. Thus the American lines cleared a dividend all round of 5.2 per cent., against 4.04 per cent. on the English lines.

The total length of railways in the world at the commencement of 1880 was calculated at:

	Miles.
Europe	102,593
Asia	8,983
Africa	3,024
America	100,867
Australia	4,338
Total	219,805

BOOK NOTICES.

PUBLICATIONS RECEIVED.

SCIENTIFIC PROCEEDINGS OF THE OHIO MECHANICS' INSTITUTE.

ABSTRACTS OF THE PROCEEDINGS OF THE SOCIETY OF ARTS.—Massachusetts Institute of Technology, 1879-1880 and 1880-1881.

REPORT TO THE NEW YORK SENATE ON THE FEASIBILITY OF UNDERGROUND TELEGRAPHY IN CITIES.

THE EDISON ELECTRIC LIGHT METER.—By Francis Jehl.

REPORT ON THE CONSTRUCTION OF TILLAMOOK ROCK LIGHT STATION.—By Lieut. Col. G. L. Gillespie.

PROFESSIONAL PAPERS OF THE CORPS OF ROYAL ENGINEERS.—Vol. 6. London. Edward Stanhope.

Among the papers are the following:
The Artillery Defence of a Fortress.
Development of Field Artillery.
Modern Rifles.
The Fortifications of Monroe.
Fortified Camps.

All of which are treated with that scientific precision and elaborate fullness for which the contributions to this journal are justly recognized.

TRANSACTIONS OF THE AMERICAN INSTITUTE OF MINING ENGINEERS. Advance sheets.

MONTHLY WEATHER REVIEW FOR APRIL. Washington: Government Printing Office.

REPORT OF BOARD OF STATE ENGINEERS TO THE GOVERNOR OF LOUISIANA.

REPORT OF THIRD MEETING OF THE MICHIGAN ASSOCIATION OF SURVEYORS AND CIVIL ENGINEERS.

METALLURGIE PAR ARMENGAUD AINE.—Paris: Librairie Technologique. Price $5.25.

This is one of a series of "Manuals." The present issue is devoted to brief descriptions of recent improvements in the manufacture of cast iron, wrought iron, and steel. The descriptions being abridged from the patent reports, are presented in chronological order down to the close of 1880.

THE EDDYSTONE LIGHT HOUSES (New and Old.)—By E. Price Edwards. London: Simpkins, Marshall & Co. Price 60 cents.

This is chiefly an abridgement of Smeaton's own account of the construction of the light house which made him famous.

It is an interesting bit of history and related a charming manner.

An account is also given of the newer structure, only just completed, together with a few illustrations of both the new and old light houses.

PETIT VOCABULAIRE RAISONNE DE MAGNETISME ET D'ELECTRICITE.—Par A. Sabourain. Paris: Journal d'Electricite. Price 50 cents.

This is a small pocket dictionary of scientific terms used in describing magnetic and electric apparatus or phenomena.

Short descriptions are given of machines or parts of machines that are known by special names.

COURS DE REPRODUCTION INDUSTRIELLES.—Par Prof. Leon Vidal. Paris: Delegrave. Price $3.50.

The different processes of picture printing are fully described and beautifully illustrated in this little hand book of 490 pages. Many of these new kinds of pictorial illustrations are called, by the untechnical, *photolithographic* pictures, thereby grouping methods of manufacture which are quite unlike.

The details of many of the new operations are so fully given that the treatise is practically an instruction book for the amateur.

EGYPTIAN OBELISKS.—By Henry H. Gorringe, Lieut. Com., U. S. N. New York: Published by the Author. Price $15.00.

This fine large quarto presents in separate chapters the following interesting topics:

Chap. I.—Removal of the Alexandrian Obelisk, "Cleopatra's Needle," to New York.

Chap. II.—The Archaeology of the New York Obelisk.

Chap. III.—Removal of the Luxor Obelisk to Paris.

Chap. IV.—Removal of the Fallen Obelisk of Alexandria to London.

Chap. V.—Re-erection of the Vatican Obelisk.

Chap. VI.—Record of all Egyptian Obelisks.

Chap. VII.—Notes on the Ancient methods of Quarrying, Transporting, and Erecting Obelisks.

Chap. VIII.—Analysis of the Materials and Metals found with the Obelisk at Alexandria.

The first chapter will be read with interest and pride by American engineers, while the untechnical reader will also find it an intensely interesting narrative.

The 2d, 6th, and 7th chapters are replete with historical information, while the 3d, 4th, 5th, and 8th, although of less interest to general readers, are necessary to a complete treatment of the subject.

There are 45 illustrations, mostly photo-engravings and artotypes.

Commander Gorringe deserves the patronage of an extensive sale of the book, and all buyers will surely get the full value of their outlay.

KNIGHT'S NEW MECHANICAL DICTIONARY.—By Edward H. Knight, LL.D. Boston: Houghton, Mifflin & Co.

Since the completion of "Knight's American Mechanical Dictionary," in 1877, the progress made in the development of the mechanic arts is unprecedented in the history of the world. Not only in such striking and wonderful achievements as relate to the telephone, phonograph, and electric light, toward which popular attention is naturally drawn, but in every department of applied mechanics, there has been developed a fertility of resource in the adaptation of means to ends quite as marvelous and equally important in practical results. Achievement has outrun the most sanguine expectation, and with such rapidity that even the most recent records are found to be very deficient in supplying the special information most desired.

The hearty approval which "Knight's American Mechanical Dictionary" has received in all parts of the world has encouraged the publishers to issue an entirely new volume, thus continuing the record from the date at which the former work went to press, but carefully avoiding repetition, and aiming to furnish not only a satisfactory supplement to the original work, but a book which shall have an individual and separate value as a complete record of half a decade in the history of invention. From this fact it is evident that this volume forms an indispensable supplement to all works of reference upon mechanics now extant, as none of them cover the period mentioned.

The same method has been adopted in dealing with the subject matter in both works. First, each article appears in its proper alphabetical place, thus fulfilling the function of a Dictionary, in affording direct response to inquiry. Second, the items of information thus distributed throughout the work

are classified in Special Indexes of the Art, Profession, or manufacture to which they pertain. The book thus fulfills the function of a Cyclopædia, which is a collection of treatises.

The value of a work of reference depends largely upon its Index. When one has a question to ask of an ordinary Cyclopædia it is frequently very difficult to determine under which title or heading to look.

The author has invented a system of what he terms "Specific Indexes," by the use of which the inquirer is guided straight to the information he is in quest of even though he be entirely ignorant of the name of a thing, and have but the most vague and general notion of its use. This is accomplished by grouping under the general title of each Science, Art, Trade, or Profession a list or "Specific Index" of every article in the book bearing any relation to the subject in question. The titles of these Indexes are in turn grouped at the beginning of the book, so that by a glance one may determine which clew to follow.

Besides the use above mentioned, these Specific Indexes afford the reader an excellent opportunity for investigating thoroughly all that pertains directly or indirectly to any special subject, by using the Index under the title of that subject as a sort of head-center, and following out its various branches through all their ramifications.

Special attention is called to a new and valuable feature in the work, by means of which exhaustive information on any subject is placed within easy reach. The author has made a complete Index to technical literature covering a period of five years, and embracing all English and American technical journals published from 1876 to 1880 inclusive. Under title of each subject may be found a complete list of every article which has appeared, during this period, in the columns of these periodicals and as every subject of importance has been thoroughly discussed therein, it is evident that the whole range of recent investigation is thus placed at easy command.

A Treatise on Rivers and Canals, Relating to the Control and Improvement of Rivers, and the Design, Construction, and Development of Canals. By L. F. V. Harcourt, C. E. Oxford: The Clarendon Press. 1882.

"Rivers and Canals," so-called in the short title on the back and on the first page, forms a useful contribution to a class of literature which is assuming considerable importance. We mean a class containing books of a comprehensive but elementary nature, the true area for the utility of which lies in those wide fields open to the engineer in the Colonies, of which we heard something the other day at the annual dinner of the Institution of Civil Engineers. Far away from cities, professional library, or senior adviser of experience to consult, the young engineer in India or Australia will find in this volume a very useful handbook. The object of the writer, has been, he tells us, to "present, in a simple and concise form, descriptions of the principal and most recent works on rivers and canals, and the principles on which they are based." In the book, however, this order is reversed. Mr. Vernon Harcourt first treats of the meteorological and hydraulic phenomena of rivers, of the measurement of river discharge, of the early and later stages of river navigation, and of the construction and supply of canals. He then enters into the practical questions of dredging-machines and appliances, of facine work, piles and coffer-dams; of foundations, of the works for affording a passage from one water level to another, of weirs, and of various works on rivers and canals. This part of the volume is clear and concise, dealing fairly and appropriately with the subject, and leaves little to desire except such a distinct reference to the authorities relied on as might be available to the student who has access to a library. Thus the expression, "it is necessary, according to Professor Rankine," (p. 41), and "is estimated by Professor Rankine," rather stimulate than satisfy the curiosity to see what are the actual words of that eminent writer; especially as to such an allowance as a loss of 2 inches of water per day over the whole surface of a canal

Ten chapters are occupied with the forementioned subjects. The eleventh chapter is a brief, hasty, and inadequate performance, in no way up to the level of the rest of the book. It is headed "History of Inland Canals." The facts stated are few, and the statements are not always accurate. Thus we find, "There are 300 miles of canals in Ireland," the fact being that there are 392 miles of canals and river navigation, in possession of companies, 133 miles under the control of local masters, and 227 miles under Public Works Commissioners—in all, 752 miles, instead of 300.

The inadequate mode of dealing with this part of the subject is the more to be regretted from the fact that where there is one man who wishes to be instructed as to the method of making a canal, there are hundreds who are anxious to know what canals are in existence, what canals are in process of construction, and at what cost traffic can be conveyed on canals, as compared to railways. It is hardly too much to say that this is *the* industrial question of the day. As such, at all events, it is regarded to a great extent by manufacturers, and discussed by Chambers of Commerce throughout England. To treat it with any approach to accuracy would require not a chapter, but a volume. Still, something useful might have been said in a chapter, and, above all, what little was said, ought to have been correct.

In the next chapter, on Ship Canals, Mr. Vernon Harcourt does more justice to his subject and to himself. The short notice of the Languedoc Canal has all the more interest from the fact that the construction of a new Ship Canal from the Mediterranean to the Bay of Biscay is at this very moment under discussion in the French Cabinet.

There is a good account of the Amsterdam Ship Canal, abstracted, as are most of the following descriptions, from the excellent author-

ity of the Minutes of Proceedings of the Institution of Civil Engineers. The account of the Fen Rivers, chiefly taken from Mr. Wheeler's "History of the Fens," is also clear, though brief. Three chapters on the improvement of tidal rivers will be read with interest and advantage. The accounts of the Liffey, the Yare, the Clyde, the Tyne, and the Tees are taken from the "Minutes." There is a want of references as to the other instances cited, but the work is done clearly and well, and Mr. Vernon Harcourt shows himself a careful abstractor. But the cases which he selects must be regarded rather in the light of vignette illustrations, so to speak, of the various methods adopted by river engineers, than as a general description of river and canal communication. So far, indeed, is the author from attempting such a work on navigation as is suppled, with reference to France, by M. Felix Lucas, in his "Etude Historique et Statisque sur les Voies de Communication de la France," that he describes the future works of the Panama Canal with as much gravity as the actual engineering of other parts of the world. And he has done so while citing on one page the unqualifiable assertion of M. de Lesseps, "that the construction of a Ship Canal across the Isthmus of Panama presents fewer difficulties than the Suez Canal," while he tells us in another page that for the latter "no constructive works of any magnitude had to be executed." Considering that the Culebra cutting of eight miles long varies from a depth of 100 feet to that of 300 feet, through a pass of the Cordillera, the idea of what constitutes engineering difficulties is not quite distinct.

The plates, which form a separate volume, are clear and good. There are twenty-one plates, all folded, and twenty woodcuts in the text. The work can be safely commended to the student, who will find brought together in its pages much for which he would have to search widely in order to collect it for himself.

MISCELLANEOUS.

THE FLOW OF LIQUIDS IN PIPES.—At the recent meeting of the Physical Society, Mr. W. F. Stanley read a paper on the flow of liquids in pipes, and showed that liquids move by rolling contact upon or past the resistant surfaces of the pipe, and not by sliding, gliding or shearing action, as has been generally assumed. The difficulty in carrying out his experiments lay in the fact that when a liquid flows through a pipe the friction of the pipe prevents the free motion of the rolling particles. For this reason with circular pipes the evidence of rolling contact is of a very complex character, and particles of solid matter, for example, descending in glass pipes take a spiral or zigzag path very difficult to follow. Evidence of surface rotation was, however, found in the descent of a liquid cylinder or column of dense mastic varnish through a tall narrow beaker from a glass funnel. The length of the descent was about 18 in., and the width of the column, 4 in. It carried down with it small particles of solid matter and tiny air bubbles, which were seen to be in rapid rotation. Mr. Stanley illustrated his theory with a number of corroborative experiments with pipes of different forms.

DESTRUCTION OF CARBON ELECTRODES BY CONTINUED ELECTROLYSIS.—Bartoli first observed that the quantity of gas generated during the electrolysis of water at the positive pole was comparatively too small, that is, less than half the volume of gas collecting at the negative pole, when this positive pole consisted of carbon. The loss could be explained by a combination of the delivered oxygen and the carbon. In connection with M. Papasogli, then, M. Bartoli further studied the matter, principally to ascertain what organic bodies would result under these circumstances. As such they determined mellitic and hydro-mellitic acids. Their experiments are, however, not less instructive from another point of view, as they show that the use of carbon as a positive electrode finally ends in the total destruction of the solid carbon. A fine powder soon collects at the bottom of the voltameter, and the liquid itself becomes more and more colored, not from sensibly suspended particles of carbon, as might be presumed, because repeated filtering and keeping the liquid undisturbed for months does not produce any change in the color. Distilled water as well as diluted solution of nitric, sulphuric, acetic, oxalic acids of potash, soda, and some carbonates, were tested with pretty similar effects. Of the three sorts of carbon employed, graphite, gas carbon and charcoal, the two latter are used somewhat quicker. One piece of carbon electrode was totally destroyed in 29 days, with 100 Bunsen elements acting for four days, 40 elements for five, and 20 elements for 20 days. Carbon may, on the other hand, be used as a negative electrode without any risk, a distinct proof that we have to deal with an oxidation process.

THE following subjects are announced by the Belgian Academy for prize competition: In mathematical and physical science: Establish, by new experiments, the theory of reactions of bodies in the so called nascent state. Prove the accuracy or falsity of the following proposition by Fermat: To decompose a cube into two other cubes, a fourth power, and generally any power into two powers of the same name, above the second power, is impossible. New spectroscopic researches required as to whether, especially, the sun does or does not contain the essential constituent principles of organic compounds. Extend, as much as possible, the theories of points and straight lines of Steiner, Kirkman, Cayley, Salmon, Hesse and Bauer, to the properties which are, for superior plane curves, for surfaces, and for skew curves, the analogues of theorems of Pascal and Brianchon. In natural sciences: New researches required on germination of seeds, especially on assimilation of nutritive stores by the embryos. New researches required on development of Trematodes, from the histogenic and organogenic points of view. New stratigraphical, lithological, and palæontological researches required, to fix the arrangement or the order of succession of

layers of the formation called Ardennais by Dumont, and at present considered a Cambrian. Medals valued at 800 francs will be given as prizes in the first division; medals of 600 francs in the second. Memoirs may be written in French, Dutch, or Latin, and should be sent (in the usual form) to the Secretary, before August 1, 1883.—*Nature.*

A SIMPLE new thermometer, said to be very sensitive, has been described (*Jour. de Phys.*, April) by Mr. Michelson. It depends on the expansion of hardened caoutchouc by heat. A very thin strip of the substance is attached to a similar strip of copper. The lower end of the double strip is fixed, and the other has attached to it a fine glass fiber bent at a right angle, through which, as the strip bends under heat, motion is imparted to a very light silvered-glass mirror, hung by a cocoon fiber. The displacement of the mirror is observed with a telescope and reflected scale, or by the movement of a spot of light. To avoid sudden changes of temperature, the double strip is inclosed in a metallic case having a slit opposite the strip. In a modification, which the author has not yet tried, the strip is reversed, and the lower end enters a highly resistant liquid, in which it faces a metallic point; the two serve as electrodes, connected with a galvanometer and a Wheatstone bridge.—*Nature.*

BY authorization of the Russian Minister of Public Instruction, the Imperial University of St. Petersburgh is about to found an astronomical observatory, which will be of small size conformably to its principal object, which is to facilitate the studies of those who are engaged in the University curriculum. The principal pieces forming the *materiel* will be two refractors, with Merz object glasses, one 6 inches aperture, the other 4 inches, parallactic mounting and clockwork motion, several transportable astronomical instruments, and an astronomical clock, with some other secondary instruments.

AT a recent meeting of the Seismological Society of Japan, Prof. Milne read a paper on the "Distribution of Seismic Activity in Japan." This paper was to a great extent founded on communications received from almost all parts of Japan in answer to inquiries respecting the occurrence of earthquakes in various districts. As the result of these inquiries, during the past two years, Mr. Milne had received, in addition to general opinions respecting the seismic activity of various districts, a very large number of actual records. Commencing in the north and proceeding to the south, notes and catalogues of earthquake intensity for the whole country were given. Thus for Hakodate, in Yezo, from 1876 to 1880, a catalogue of forty-two earthquakes was given. By comparing this catalogue with that of Sapporo, in the same island, it was seen that ten at least of the Hakodate shocks had been felt at Sapporo, eighty miles to the northeast; and similarly it was shown that seven of the shocks were felt at Tokio, five hundred miles to the south. From the times at which a shock was felt in different localities, its intensity and the like, origins for certain shocks were roughly computed. The district around Tokio is of course that which is being most thoroughly investigated; and as it was only possible to obtain *accurate* observations as to the time at which shocks were felt at one or two localities, and farther, as it was shown that the direction in which the earth moved at any given point as indicated by a seismometer did not necessarily indicate the direction from which the earth waves were advancing, Mr. Milne has adapted the following simple method as an assistance in tracing earthquakes to their origins. All important towns within a radius of one hundred miles from Tokio have been furnished with bundles of post-cards, one of which is posted every week stating whether earthquakes have or have not been felt. In this way, at the end of last year, Mr. Milne found that the greater number of the earthquakes which were felt in Tokio had only been felt in the towns to the north of that city, and a short distance to the south. This fact being established the barrier of post-cards was continued about two hundred miles still farther north, with the result of enclosing, so to speak, the origin of several shocks, and tracing others to the seashore. The latter could no longer be pursued by means of post-cards, and instrumental observation alone had to be relied on for the determination of their origin. These observations, so far as they have at present gone, show in a remarkable manner how a large mountain range absorbs earthquake energy. Thus, it is very seldom that an earthquake traveling from the north passes beyond the Hakone range of mountains to the south of Tokio. Earthquakes having their origin on either side of such a range rarely travel to the other side, however large their area of activity on their own side may be. The whole of Japan has in this way been divided into districts of varying seismic activities. By two separate systems of investigation Mr. Milne showed that, if instruments of ordinary sensitiveness were distributed throughout Japan there would on the average be recorded, at the lowest estimate, over 1,200 shocks per year, or about three shocks per day, which is a number greater than that obtained by Prof. Hein for the whole world.

A NEW dynamo-electric machine, recently brought before the Belgian Academy by M. Plucker, has the peculiarity that a solenoid is substitued for the electro-magnet as an organ for excitation of the induction currents. The horizontal coils of the solenoid, which is of special form, are traversed by the currents produced by the machine itself. The apparatus rotated within the solenoid is a wheel with coils arranged nearly like those of the Gramme ring. The whole system is enclosed in an iron armature meant to increase the inductive action. M. Plucker states that he replaced the solenoid with electro-magnets, and the apparatus produced the same effect. He seems merely to claim the advantage of less weight and volume.